Knowledge & Power

Lectures by F R Jevons

Edited by C E West
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Knowledge & Power

Lectures by F R Jevons
The 1975 Lectures of The Australian National University Advisory Committee on Science and Technology Policy Research
Edited by C E West

The Australian National University Advisory Committee on Science and Technology Policy Research
Canberra 1976
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Foreword

The Australian National University was delighted to invite Professor Jevons to visit Canberra in June and July 1975 to give a series of lectures. The visit was organised by the Australian National University's Advisory Committee on Science and Technology Policy Research with the aim of stimulating interest within the University in the academic study of science and technology policy issues. This book contains the text of four of Professor Jevons' lectures, in Chapters 1, 3, 4 and 5. Since no record was taken of a further lecture, an article on the same subject which he published during his visit, in The Canberra Times is included here as Chapter 2 by kind permission of the Editor. Chapter 6 is the text of a talk Professor Jevons gave on the ABC Radio Guest of Honour programme. The discussions which followed the lectures have also been included, and in chapter 7 there is an edited transcript of a general discussion held in Canberra towards the end of his stay.

At the time his visit was arranged Professor Jevons was Professor of Liberal Studies in Science at Manchester University. To the great delight of the friends he made during his visit to Australia, he accepted appointment during his stay as the founding Vice-Chancellor of the new Deakin University which is being established at Geelong in Victoria. We, at the Australian National University, are very glad to be able to publish some of the fruits of his stay, and are delighted to have this opportunity to extend to Professor Jevons and the new Deakin University the best of good fortune for the future.

D. A. Low
Vice-Chancellor
The Australian National University
Canberra, March 1976
Introduction

The development of defined science policies (that should be taken to include technology policies) and academic studies which might support their formulation have not received much attention in Australia until recent years. In government there was for a long period of time, opposition to the establishment of an overall science policy or of machinery for developing such policy. Thus, Mr Gorton, when Prime Minister in 1968 said 'I don't know what a science policy is. The critics want an overall advisory committee to allocate funds, but I don't see the need for any advisory body. These committees are only a group of individuals pushing the barrow for their own discipline.' Since that time several developments have taken place suggesting that a series of defined policies for science and technology could possibly emerge in Australia. These developments include the establishment of the Australian Government Department of Science and the interim Australian Science and Technology Council (ASTEC) which will advise the Government on science and technology policy matters. However a large number of observers would agree that the Department of Science has made little positive impact on the conduct of science in Australia since it was created in December 1972 and now the future of ASTEC is up in the air with the appointment of a committee to advise the Prime Minister on its future. A panel from the Organisation for Economic Co-operation and Development has submitted a report on Australia's science and technology activities. It is also expected that the final report of the Royal Commission on Australian Government Administration foreshadowed for April 1976 will devote considerable attention to the organisation of science within the Australian Government as the Commission established a Science Task Force which reported in November 1975.

On the academic scene, it is only in recent years that appointments have been made in an Australian university in the science policy area or, to be more correct, in the broader study area of science, technology and society (at Griffith University, Brisbane). However courses have been given and valuable contributions to the literature made by people whose prime interest is in other areas. Such people include Sol Encel (University of New South Wales),
Leon Peres (University of Melbourne) and Ann Mozley Moyal (New South Wales Institute of Technology). Many people in industry have also published thoughtful and well-researched papers on science policy issues, mainly in Australian journals such as Search and the Proceedings of the Royal Australian Chemical Institute. In addition, groups such as the Australian and New Zealand Association for the Advancement of Science (ANZAAS) have established committees which have made useful contributions to the discussion of science policy issues in Australia.

Despite the present chilly climate for the establishment of new activities within universities both in Australia and overseas, there will be, and should be, moves to establish courses and promote research on the relationships between science, technology and society. I think it is important that these new ventures manage to develop an academically critical approach to the subject whilst not becoming alienated from other groups especially those conducting research. Such alienation could quite readily occur for three reasons.

Firstly, recruitment of staff to the new ventures should not be restricted to those associated with existing courses in the area either as teachers or students. Those with immediate backgrounds in the natural sciences but with a strong interest in the new study area should fill many of the new positions. Such an approach was adopted in the early days of the Department of Liberal Studies in Science at Manchester University (Chapter 4). One of the functions of the establishment of these new units would be to allow scientists to switch to the study of science and technology policy issues. There would also be scope for the employment of people in the social sciences. If graduates and teachers of the existing courses dominate the new ventures, the ventures run the risk of becoming entirely derivative. It is also possible that such a development would accentuate the problems outlined in the next two paragraphs.

Secondly, people in the new ventures should not feel that they possess all the skills and knowledge for their studies within their own resources. They must liaise closely with scientists who are often their research material and social scientists who often have the skills to attack the problems. Thus Professor Birch, who was associated with the development of the contraceptive pill was not impressed with the American study of that development. 'They had all the background wrong', Professor Birch said (Chapter 1 — discussion). Similarly Professor Harris' comments on the research in Manchester on innovation responses to the mercury scarcity predicted by Meadows et al. in The Limits of Growth would suggest that those involved were
initially not cognizant of the pitfalls associated with studies of commodity price formation as would be economists specialising in this area (Chapter 4 — discussion). In other words we have to guard against the creation of ‘cliff-effects’ not only between scientists and social scientists as Jevons suggests (Chapter 3) but also at the interfaces between these two groups and the new breed of ‘science policy’ academics and students.

Thirdly, I think there should always be a place for ‘amateurs’ in the study of science, technology and society. By amateurs I mean those people not working in the area full-time or at all such as bench scientists, economists, sociologists, philosophers, industrialists and politicians. Natural scientists in particular often gain the impression that academics studying science policy (not Jevons) show a degree of antipathy to amateurs venturing into the area. Perhaps some of the contributions by the amateurs may not be soundly based or may be inspired by ‘pushing the barrow for their own discipline’ but I do not think that such shortcomings would be restricted to their work. The full-time devotees of the subject will probably make their greatest contributions to those academic studies requiring the collection of a lot of data and background information while the amateurs will make their mark by criticising the studies of the full-time workers and by exploring entirely new fields of study. Thus efforts to over-institutionalise the study of science, technology and society should be resisted as much as possible (after repeating the words ‘the study of science, technology and society’ so many times, I am tempted to use the word ‘scienomics’ introduced by the late Chief of the CSIRO Division of Plant Industry, Dr J. E. Falk, but that would probably concede complete institutionalisation of the subject). One way of reducing the institutionalisation pressure would be to place more emphasis on educational programmes at the Masters’ and postgraduate diploma level rather than at the undergraduate level. This approach is widely advocated for interdisciplinary studies in the environmental area (Chapters 3, 4 and 7). Masters’ and postgraduate diploma studies also cater for people with work experience in science and science policy whilst undergraduate courses tend to concentrate on school-leavers.

A corollary of the maxim that ‘amateurs’ have a role in science policy studies is that amateurs should have a role in science policy decision making processes. It would seem to be desirable that overall science policy decisions should not be entirely in the hands of bureaucrats in rigidly structured organisations but that people from a wide variety of backgrounds should be involved. Thus there is
merit in having a body, structured along the lines originally proposed for ASTEC with members from a wide variety of backgrounds such as philosophy and the trade union movement, participating in science policy formation.

There is probably no better advertisement for a department of full-time devotees to the study of science, technology and society than the Department of Liberal Studies in Science established by Jevons at Manchester University. If more departments can be established with the same success, the community will be well served. The graduates from Liberal Studies in Science have been readily accepted by employers and the research has contributed to our understanding of science from the economic, social, historical and philosophical standpoints. This book outlines some of the academic contributions of Jevons and other members of his former department in the field of science policy and related matters involving the interaction of science with society. The topics covered in the book range from the relationship of basic scientific research to technological innovation in industry, through the nature of knowledge to a discussion of science and literature.

From his experience at Manchester University and his observations in Australia, Jevons has provided useful suggestions for the type of research that could be carried out in an Australian setting. The topics suggested include the organisation of scientific research in Australia especially the flows of information, international comparisons of research particularly with Canada, industrial research management and technology assessment. Adequate study of these topics involves a lot of work usually beyond the resources of the people I have referred to earlier as 'amateurs'. Perhaps somebody may even examine Jevons' postulate that study leave for Australian academics might in some cases do more harm than good because academics spend a lot of time travelling and not staying at home using the research facilities available in Australia. I wonder if the research topic will be examined by means of 'rational thought' as outlined by Professor Robertson. Such thought demands that the results of an investigation are interpreted 'without prejudice arising from our particular backgrounds' (Chapter 7).

C. E. West
Canberra, February 1976
References


First of all I would like to thank you all very much for inviting me out here. Having been here all of ten days, I think Australia is a wonderful place. As for science policy, I'm very impressed by the lively and wide interest taken in this field in Canberra and in other parts of Australia. The last thing I would want to do, or indeed could do, is to tell you how to run Australia's science policy, especially in view of the turmoil there has been since I arrived. I deny the possibility of any causal relationship, but since I arrived the science policy field seems to have been thrown into the melting pot. What I hope I might contribute is something on the background which academic studies might be able to provide for science policy discussions. Not too much should be expected: scholarship is one thing, practical policy making is another. But there are points of contact, or at least there should be. Academics, I feel, should keep one foot on the ground (not more than one, not less than one — one is on average the right number!).

One problem in science policy which exercises science policy people in many countries — not only in Australia but also, for instance, in Canada — is the question of the relations between research and industrial needs. In particular, what is the relationship of basic scientific research to technological innovation in industry? What can science do to stimulate the wealth-creating function in an industrial state?

I'd like to talk today about some work which has been done in my department in Manchester University, the Department of Liberal Studies in Science, on the role of science in industrial innovation, or, to put it in slightly different words, on the science-technology relationship. Those two words, science and technology, are commonly used loosely. Science is often taken, especially in everyday speech, to include technology. When the two are distinguished it is sometimes with a somewhat snobbish bias in favour of science. That bias is perhaps somewhat less strong now than it was ten years ago
but it still exists. Some of you may remember the joke, dating from the days of the space race, about the moon shot which was a success being hailed as a scientific triumph, whereas the one that failed was excused as merely an engineering failure!

We could discuss the meanings of the words *ad infinitum* but that would not be very profitable. The general distinction seems to me quite clear. Science is knowledge and understanding of the physical universe, whereas technology is the capability to do things — to make materials that withstand high temperatures, to go to the moon, and so on. Through all the difficulties of interpretation and of demarcation that arise, that seems to me to be the basic distinction. Thus the question of the science-technology relationship really boils down to a discussion of the role of understanding increasing human capabilities. How does it increase our capabilities to understand how the world works? How does knowledge of nature give us power over nature?

The idea that knowledge of nature might give us power over nature is not new. It was formulated early in the 17th century by Francis Bacon3 who wrote in his *Novum Organum* Book 1, aphorism 3: ‘Human knowledge and human power meet in one, for where the cause is not known the effect cannot be produced.’ Human knowledge and human power are seen here as inseparable, as virtually one and the same thing. And Francis Bacon was of course a very influential writer, partly because he wrote such superb English. He was a prophet, perhaps the prophet, of the use of organised research to acquire power over the material universe, or, to use his own phrase, ‘for the effecting of all things possible.’ This notion was developed in his delightful fantasy *The New Atlantis* and partly put into effect some forty years after his death in the creation of the Royal Society of London in the early 1660s.

It is a fine-sounding idea that human knowledge and human power meet in one, but a moment’s thought shows that it cannot be more than a half-truth. Everywhere, all the time, we do things because they work, even though we have little idea why they work. We do have power without the knowledge. The effects are produced even though the causes remain obscure. Penicillin cured diseases long before we knew its structure, let alone its mode of action. Polymers were synthesised and used long before the notion of giant molecules was accepted.

Nevertheless, the Baconian notion has always been a popular one within the academic community, largely because it gives to the academic community such a key role. Academics like repeating
stories like this one, which I have heard attributed variously to Faraday and to Benjamin Franklin, largely, I think, according to whether it is told in England or the United States. Faraday, or Franklin, as the case may be, was asked by a politician what was the use of all the work he was doing on electricity. The reply was, 'What's the use of a baby?' It might grow up into anything, it has an infinite range of potentialities. (There are various versions of the story. In another version, the scientist's reply to the politician was 'Some day you will put a tax on it'.) Such stories, however, don't prove very much. They tell us something about the self-image of scientists but they prove nothing about the realities of the science-technology relationship. As recently as 1967, the Council for Scientific Policy, which was the top science policy body in Britain from 1965 until 1972, said in its second report, 'Basic research provides most of the original discoveries from which all other progress flows'. That is a big claim! I was reminded of it last week when I heard Sir Hugh Ennor, Secretary of the Department of Science in Canberra, refer to 'the incontrovertible fact' that 'tomorrow's technology depends on today's science.'

**Benefits from research**

It was from the Council for Scientific Policy that we in Manchester derived the stimulus which focused our interest on this problem. In 1969, a paper was produced by Byatt and Cohen called 'An Attempt to Quantify the Economic Benefits of Scientific Research.' Byatt was a senior economic adviser to the Department of Education and Science and Cohen was the Scientific Secretary of the Council for Scientific Policy. So this was a joint production by an economist and a scientist turned administrator. I am going to criticise the Byatt and Cohen approach but I should say at once that it was a thoughtful paper which served the purpose for which it was written, which was to stimulate concrete investigations in the field.

The context which gave rise to the paper is one of the central, crucial science policy issues. Why should the taxpayer pay for science? What are the justifications for public support of research? The economic benefits which might or might not arise are not the only possible justification, nor is there only one route by which economic benefits can arise from scientific research. Byatt and Cohen summarised the kinds of benefits one might envisage arising from scientific research under four headings. There is a more detailed classification in their paper but the four main headings are:
1, manpower; 2, mission-oriented; 3, curiosity-oriented; and 4, cultural. Let me take them one by one.

The manpower benefit is the benefit which, it is supposed, arises via the students who are taught by those who are active in research. It is held that because university students are taught in universities by people who are active in research they are taught better. What sort of evidence do we have for this? There have been hundreds of statements testifying to the vitalising effect that research has on university teaching. Hundreds of university teachers have said, 'Our teaching would go stale if we didn't also do research.' On the other hand, there are also hundreds of statements to the contrary. Some of them have come from students, some have come from staff employed in non-university institutions which place less emphasis on research. Many of these statements argue that teaching might actually improve if the staff regarded themselves as primarily full-time teachers rather than research workers doing a bit of teaching on the side. As far as I know, we have only assertion and counter-assertion to decide this issue. If you can tell me a way to investigate it, a way to do a properly controlled experiment on it, I would be delighted to hear it. In the absence of such an investigation, we can only assess claims and counter-claims. So a question mark hangs over benefit number one, the manpower benefit.

Let me take number four, the cultural benefit. Culture seems to be used here in the old-fashioned sense rather than the new sociological sense; it seems to mean high culture, science being compared with opera, cathedrals and so on. Most people will agree that there is a valid benefit here. There is a cultural benefit from knowing more about science and knowing about atoms, cells, evolution and so on. How much of a benefit is this? How much is it worth in hard cash? I have some figures here that related to Britain, for 1972-73 and they show that roughly the research councils (the Agricultural, Medical, Natural Environmental, Science and Social Science Research Councils) which give money for basic scientific research in Britain, get about ten times as much money as the Arts Council. If you try to get more comprehensive data the situation gets complicated because of course the research councils are not the only source of money for research, nor is the Arts Council the only source of money for the arts. I suspect, however, that the answer would come out roughly the same: an order of magnitude greater for science than for arts. The figures for 1972-73 are that the research councils between them got £stg120m, the Arts Council got £stg12m. If you add other channels of government support, the order of magnitude difference remains.
That order of magnitude difference is difficult to justify on the basis of cultural benefit. Even if you accept that science is beautiful, which it is, you have to admit that it is also rather esoteric and that therefore the audience for it is limited in size. It seems hard to justify, on cultural grounds, spending ten times more taxpayers' money on science than on arts.

That leaves us with benefits two and three, the benefits from mission-oriented and curiosity-oriented research respectively. The distinction that Byatt and Cohen made between these two has to be noted carefully. Mission-oriented research they defined as ‘research in fields whose application is evident, for instance work in agriculture and medicine’. This is research where some application is evident, so there is no conceptual difficulty about how the benefits arise. It may be fiercely difficult to calculate the magnitude of the benefits because from any particular innovation there are indirect secondary effects and higher order effects; nevertheless, the route is clear because the success of a project which has an applied objective leads directly to the benefits. If, for instance, you set out to breed a new variety of hybrid corn and you succeed, then it is through the hybrid corn that the benefits arise. If you set out to make an oral penicillin and you get a penicillin that can be taken orally, it is through the manufacture and use of the new penicillin that benefits arise. No conceptual difficulty here about the mode of the payoff.

The same thing cannot be said about benefit number three, the benefit from curiosity-oriented research. Curiosity-oriented research was defined by Byatt and Cohen as research which leads to basic ideas which, they say, tend to arise unpredictably. Here, the mode of payoff is itself the principal uncertainty. How do basic ideas which arise unpredictably lead to benefits? The background to this question was of course the general disenchantment with science which began to set in during the late 1960s, the more insistent questioning of whether it is worth spending so much money on science, and in particular, in Britain, the debate over the 300 GEV particle accelerator which physicists wanted to build in Geneva. This enormous machine was to be built as an international project at a total cost of £stg150m of which Britain would have contributed about a quarter. Nobody denied that it would lead to good physics. It was going to give us the most fundamental information; there is no information more fundamental than information about the structure of the matter we are all made of. But nobody, not even the physicists, could think of any foreseeable practical application for this work, and people naturally wanted to know why we should
spend £30m to £40m of our money on this sort of work.

It was against that background that Byatt and Cohen suggested a method for quantifying the economic benefits from curiosity-oriented research — benefit number three, the most difficult of the four. They suggested estimating the total economic surplus from a given innovation. This should really be done for society as a whole, but in particular cases the economic surplus for an individual firm might be a reasonable approximation to that. For a firm the cash flows might look a bit like those in Figure 1. At point D in time, a discovery is made by a curiosity-oriented researcher — one of those basic ideas that arises unpredictably and looks as it if might have application. Money is then invested in applied research. If that looks promising, more money is invested in development, and if that looks promising an even bigger capital investment is made to set up a factory with machinery to make the product. Typically, those capital costs are greater by an order of magnitude than the research costs, the costs of the applied research and development. Once the factory has got going there are also running expenses. All the solid lines in the diagram represent outgoings. If the innovation is successful there will then be some returns. The dotted lines represent the money that comes in from sales and perhaps also from licensing the process to other firms. All these cash flows must be discounted to some common year to give the net surplus from that innovation. Discounted cash flow is basically a very simple technique to eliminate the time factor — it just means applying compound interest to all the money values to bring them all to a common year.

Figure 1
Now imagine, said Byatt and Cohen, what would have happened if discovery D had not been made until a few years later because less money was being invested in basic research. (Note the assumption that if you invest less money in basic research the same discovery will be made but a few years later; that is a questionable assumption but it is the simplest one to make). D then becomes displaced and there would be a change in the surplus. Usually, said Byatt and Cohen, the surplus would be smaller. It would not always be smaller, but Byatt and Cohen suggested that a delay in D would usually decrease the surplus. The change in surplus gives the value of that increment in support for basic science which enabled you to make the discovery earlier rather than later. The method uses the ‘marginal’ approach dear to economists; it depends on a notional marginal delay in the making of a discovery due to less support for basic science.

There is embodied here a particular view of the relationship between science and technology. The implied model of that relationship is that technology is science applied: science discovers something, technology applies it. Moreover, a temporal as well as a cognitive relationship is implied. The scientific discovery determines not only what innovation takes place but also when it takes place. The timing of the innovation is determined by the timing of the discovery. D is the trigger which sets off the sequence of events which leads to industrial innovation and economic surplus.

Test of the Byatt/Cohen method

Byatt and Cohen only suggested the method. They wanted people in universities to test it and sent their draft paper to some interested bodies including my department in Manchester. We never believed that industrial innovation works like that, but we felt that if we just wrote back by return of post and said so they would take no notice. We therefore graciously consented to accept a small research grant from them in order to investigate their proposal. This we were able to do because we were already doing a study of industrial innovations. This study, now published in full in the form of a book called Wealth from Knowledge took a sample of 84 successful technological innovations, the sample having been selected independently of us by the Queen’s Award Scheme which was started in Britain in 1966 to reward success either in technological innovation or in exports. We were not interested in the export awards but we looked at all the awards for technological innovation made in the first two years of the scheme, 1966 and 1967. We
thought we could use this sample to try out the Byatt-Cohen suggestion. Our conclusion was that it is hard to find a case which fits the Byatt-Cohen model for the very simple reason that you cannot usually pinpoint a discovery D on which the timing of an innovation depends. Let me give just one example to illustrate this.

The Chorleywood Bread Process (the name comes from the place in England where the British Baking Industries Research Association is situated) was announced in 1961. It was an extremely successful innovation. By 1969, two-thirds of the bread baked in Britain was made by this new process. It is a way of speeding the development of the dough. In the traditional way of making bread, flour is mixed with water and yeast and a few other additives and left for a few hours to 'develop'. During that development period fermentation takes place, and 2% of the dry weight of the flour is lost as carbon dioxide, and certain physical changes take place in the wheat protein. In the new method, the development period is cut down to a few minutes by a controlled amount of mechanical work plus the addition of chemical 'improvers'. This avoids not only the delay, which costs money, but also the 2% weight loss. There are also other benefits in that more home grown wheat can be used instead of expensive imported Canadian wheat. The controlled amount of mechanical work is put in by 'overmixing', leaving the dough in the mixer longer than you otherwise would have to. When my colleagues looked into the history of that they found that the effect of mechanical work was described way back in 1926 by two workers at the Kansas State Agricultural College in the United States. At that time, no commercial process resulted. With hindsight it is not difficult to see why, because chemical improvers are also necessary. The chemical improver used in the Chorleywood process is ascorbic acid, vitamin C. (Actually more than one is used, a slow improver and a fast improver; ascorbic acid is the fast one). Ascorbic acid is of course now available synthetically. It became commercially available in 1937. The availability of ascorbic acid clearly depends on organic chemistry, some of which may well have been of a curiosity-oriented kind. We therefore asked ourselves, can the availability of synthetic ascorbic acid be regarded as a curiosity-oriented discovery on which the timing of the innovation depended? The answer to that question, however, was no. If ascorbic acid had not been available, other oxidising agents could have been used instead — potassium iodate, for example, seems to work just as well as ascorbic acid. (I suspect the choice of ascorbic acid was determined largely by the fact that it sounds better to be adding a vitamin than something with as
chemical a name as potassium iodate.) There is therefore no identifiable input from curiosity-oriented research, delay in which would have led to delay in the innovation — no trigger D which determined the timing of the innovation.

Even when we looked beyond the Queen’s Award sample, we could only think of two cases in the 20th century where innovations arose from research which was clearly curiosity-oriented — nuclear power and silicones. Rigorously interpreted, curiosity-oriented is a very restrictive definition. Any possible application in the mind of the researcher puts the work outside the curiosity-oriented category. Within that restrictive definition we could only find no cases other than nuclear power and silicones except for the transistor which is a marginal case. Nuclear power could not be used in the Byatt/Cohen scheme because the timing of the advent of nuclear power stations was affected overwhelmingly by the tremendous, cost-no-object effort which went into the development of the atomic bomb. As for silicones, they were first prepared in the laboratory by Kipping in the first decade of the 20th century but the first industrial preparation was done by Rochow working for General Electric in the United States in 1940 and the first commercial production was by Dow-Corning in 1943. So that innovation also is clearly not of the Byatt/Cohen type. What these two innovations do illustrate is the sad but apparently unavoidable fact that as a stimulus to technological innovation there is nothing to beat a good big war.

**Push and pull models**

The flaw in the whole Byatt/Cohen approach is the emphasis on ‘discovery-push’ type thinking — the idea that the origin of the innovation is a scientific discovery. According to the alternative view the trigger for innovation is the recognition of a need which scientists and technologists set out to fulfil. That is the ‘need-pull’ view — the notion that innovation is pulled from ahead by the goal to be obtained. We made an attempt to estimate the relative importance in our sample of innovations of these two mechanisms, the discovery-push and the need-pull. It was difficult, because innovations typically result from more than one chain of events — several different chains of events usually converge on a single industrial innovation. We tried to simplify the situation by concentrating on the Award-winning firm or research unit which brought about the innovation, asking the question ‘what stimulated the Award-winning firm into the action which resulted in the successful innovation?’ To
that simplified question we could give an answer in most cases. We divided the discoveries into scientific and technological discoveries, scientific discovery being of the Byatt/Cohen type and technological discovery more applied — more like an invention than a discovery. By introducing that category we got around the very restrictive definition of 'curiosity-oriented' work which excluded most research. We also made two categories of need-pull, according to whether the needs were customer needs, such as needs recognised by market research, or other kinds of need recognised by management, such as the need to reduce costs in a particular part of a process, or the need to avoid a takeover. Of the 84 cases in our sample, 8 were not classified due to inadequate information, and 35 were dual in that more than one of the stimuli was important. That gave 111 instances of stimuli. These, we found, split very neatly 2 to 1 in favour of pull-type stimuli — 74 pulls to 37 pushes. Pull was important just twice as often as push.

Having done that exercise, we were not terribly proud of it; there was necessarily a large measure of arbitrariness in the classification. We felt that we really had to re-think the problem — some sort of reconceptualisation of the whole process was necessary. It seemed increasingly clear to us that an innovating organisation is as much the seed bed as the fruit of inventive activity. It is not often that an invention comes first and is followed only later by the creation of an innovating organisation. Rather, innovation proceeds within some organisation and inventions are made during the course of that activity as technical problems arise and need solution. Because of that it seemed to us inadequate to say, as many economists have said, that innovation represents a set of investment opportunities arising from invention. According to this view, research workers produce a series of inventions; an economist or manager then does some cost-benefit sums on them; according to the figures he gets, he says, 'I'll have that one, please.' That seems to us to be an inadequate model because invention so often takes place during development, not prior to it. The causal relationships between invention and innovation are mutual, not one-way. Each is part cause and part effect of the other.

Information inputs to problem solving

Two of my colleagues in Manchester therefore started a new study. They were Michael Gibbons, who had participated in the Wealth from Knowledge study, and Ron Johnston, an Australian who had
recently joined us. The focus of attention in this new study was on the information inputs to technical problem solving in innovating organisations: not merely on the origins of innovations, but on the sources of information used in solving the problems that arose during the course of development of the innovation. Gibbons and Johnston took a sample of 30 British new product innovations. These were very recent innovations, got from the current issues in August 1971 of technical journals — quite down to earth technical publications, not scientific ones like the *Journal of the Chemical Society*, but technical periodicals like the *Food Trade Review* or *Engineering Materials and Design*. For each innovation they then identified the person or persons principally concerned with solving the technical problems. This gave a sample of 49 problem solvers, with each of whom a structured interview was conducted.

Because the innovations were so recent, very detailed information was available on the way the innovations had come about, as is indicated by the fact that an average of 30 information inputs per innovation were identified: a very far cry indeed from the approach which seeks one discovery behind each innovation. 30 innovations with an average of 30 inputs per innovation gives about 900 information inputs altogether (I am rounding the figures for simplicity but the exact ones have been published in *Research Policy.*9) The 900 information inputs were divided into three groups: the *personal*, items of information that the problem solver had already available within himself at the start of the innovation process; the *internal*, items that he got from within his firm; and the *external* items that he got from outside the firm. The 900 information inputs divided roughly equally between these three categories. The 300 external ones are the ones of most interest to us here. Of these, roughly 200 were classified as technological, 100 as scientific.

To summarise, out of 900 information inputs altogether, 300 came from outside the firm, and of those 300 one third were classified as scientific rather than technological. Here then, at last, is a significant science input into innovation. There is inevitably an element of arbitrariness in the classification, but the estimate can be regarded as a minimum one because of possible scientific inputs in the personal and internal categories which it was not feasible to identify and itemise. The scientific input to industrial innovation identified by Gibbons and Johnston is very different in kind from the input that Byatt and Cohen had assumed. The role of science is not initiating but supporting; it is not to start the innovation but to help
it to reach fruition. The originating stimulus is usually the recognition of a need to be met.

The 100 external scientific inputs came by various routes. Once again they are divided into three categories, literature, contacts with scientists, and a miscellaneous 'others' category, and once again the three groups are of roughly equal size. Regarding the literature inputs, let me comment on their age. There is a hypothesis, due to de Solla Price\textsuperscript{10} and called the 'educational cycle' hypothesis, according to which basic research is incorporated into industrial technology only after it has been incorporated into the educational curriculum. The implication is that the industrial scientist does not learn after the end of his formal education but uses only what he was taught. I am glad to be able to say that what Gibbons and Johnston found did not support this rather cynical view. On the average, the age of the literature that the problem solvers used was less than the average time since they left the educational system. The average age of the literature items they used was 12 years whereas the average time since they ended their education was 17 years. Most of the items therefore had been published after they left the education system. Clearly the problem solvers had not lost their capacity to read scientific journals.

The contacts with scientists were largely person-to-person contacts. Occasionally these arose through the employment by the industrial firm of an academic scientist as a consultant, occasionally through the support of research in the university by the industrial firm but more often they came from informal requests for advice or assistance or the use of specialist instruments or libraries in universities. The role that the academic scientist tended to play was not to provide the basic idea on which the innovation depended; rather, the academic scientist tended to respond to a problem which was put to him by the industrial man. Sometimes he was able to suggest alternative ways to tackle a problem, sometimes to provide equipment, sometimes to indicate the location of specialist information or facilities. For instance, one of the innovations was the development of a new tungsten-halogen lamp system for cars. The problem was to introduce phosphorus and halogen into the lamp, and phosphonitrilic halides seemed to be a suitable form in which to introduce them. In initial tests, however, the lamps soon went black. Chemists at a near-by university suggested that the blacking was not due to decomposition of the phosphonitrilic halide itself but caused by impurities in the sample. Furthermore, they were able to provide a sensitive analytic technique for detecting those impurities. With
pure samples, development of the innovation could proceed rapidly.

Conclusions

What general conclusions can I draw from all this? In the effort to get more value out of research, there is a limit to what can be achieved merely by telling the universities to make their research more applied and more 'relevant'. Often the proper place for fully applied research seems to be in industry itself. The onus for putting science to use lies more on the industrial scientist than on the academic. At least, it seems to be more feasible that way round, for the simple reason that it is more feasible given a practical problem, to identify the kind of knowledge which might help to solve it, than it is, given a discovery, to suggest uses for it. This is implied also in the story about Faraday or Franklin; a discovery is like a baby in that it might grow up into almost anything.

Back in 1970, when I was writing about the science-technology relationship in the preface to *Wealth from Knowledge* I put it like this: 'Perhaps science is not the father of technology but an anonymous well-wisher who sends it gifts through the post, as it were.' That really amounted to little more than a confession of ignorance; coupled with a hope and a belief that in some way, indirect and difficult to identify, science does make a contribution. As a result of the Gibbons/Johnston study, I can now be somewhat more specific and I would like to change the metaphor into the following: 'Science is not the mother of invention but nursemaid to it; that is, she helps innovation to grow up. Moreover, she depends for her livelihood on making herself felt to be useful in this way. But she does not beget innovation, except very occasionally, illegitimately, under the backstairs, so to speak.' What I mean by that analogy is that science acts in a supporting rather than in an initiating role. It supplies things like manpower, techniques and advice which are important for success in technological innovation.

By making this sort of contribution of science to technology more explicit, the Gibbons/Johnston study has, I think, brought us closer to a realistic appraisal of the role of science in the creation of wealth and the attainment of other practical social objectives. That role has sometimes been exaggerated in the past. As I suggested at the beginning of my talk, science has on occasions been 'over-sold'. It is not the fount of all progress. But one can go too far in running science down. There were some careless readers of our critique of the Byatt/Cohen approach who felt that we leaned in the opposite
direction and that we were joining the anti-science camp, suggesting that science never leads to any benefits. That was clearly not our intention. All we wanted to do was to move on from affirmations of faith to assessment of evidence. That evidence shows, we believe, that science does make substantial contributions, not in the simple and obvious form which is so often assumed but in more subtle and elusive ways. This is an important fact to recognise. If you are arguing for public support of science, it is better to base your arguments on mechanisms that can be shown to exist than on mechanisms that cannot.

Discussion

Wallington: I would like to ask a question on the inputs into innovation. The 900 inputs were divided into 3 categories and it is in one of these categories that you have been able to present the results as 2:1 in favour of technology. Although it may be impossible to determine the ratios in the other two categories, it could presumably turn out to be the other way round.

Jevons: Yes. That is why the estimate of the science input is a minimum estimate. The ratios are critically dependent on the definitions of science and technology.

Ronayne: You mentioned the First Report of the Council for Scientific Policy but in the Third Report they said 'Curiosity-oriented research is only rarely the mainspring of substantial innovation'. So they have changed their minds over the years.

The impression that an attack on university research was made at Manchester is probably due in some measure to one particular member of your team (Langrish) who has an anti-university bias. In an article in Nature last August12 he concluded that the small contribution from university science to innovations and the lack of interest shown by industrial chemists in publications by academic chemists should make it more difficult for the policy makers to think that basic science makes most of the original scientific discoveries.

Jevons: I do not think there is any conflict there because Langrish is talking about ideas from the chemical literature and that is a kind of input that Gibbons and Johnston say doesn't occur very often.

I would like to cap your quotation with this one from the Third Report of the Council for Scientific Policy. In it the Council
reported the activities of its Working Group on Economic Benefits, through which the research I have described was supported. Reading the Third Report gave us the gratifying feeling that our academic work had actually had some impact on people in policy circles. The Working Group was obliged to conclude that ‘The linkage between technology and basic science is more subtle than that proposed in the Byatt/Cohen paper. Indeed a significant part of science may well be concerned with the closer understanding of already exploited phenomena. We cannot see that it is possible in any systematic way to trace important industrial applications of science back to basic work of the kind that the Research Councils support in a way which could help to determine how much support is justified . . . We would now place less emphasis on wholesale importation of isolated scientific developments, and more emphasis on a generalised diffusion of knowledge which can take place in particular by industrial recruitment of trained personnel and by other means. A study of these mechanisms will be valuable in determining the institutional changes which would improve this process.’

*Whyte*: What substance do you think there is in the Byatt/Cohen assumption that delay in discovery leads to less benefit?

*Jevons*: It is not correct in every case. To be fair to Byatt and Cohen, they didn’t claim that it would be. In one of the cases we looked at, it seemed quite possible that a delay might have worked the other way round: the discovery might have been economically more successful had it been made later.

*Whyte*: So there is not much argument there for pumping more money into scientific research in order that we could reap great benefits later.

*Jevons*: No. I am struck by the benefits you can get by sticking to old technology. The fact that the fare on a Melbourne tram is only 10 cents impressed me very favourably; you cannot go very far for the equivalent of 10 cents on a Manchester bus now.

*Cliff*: As far as I can see, the most crucial factor here is the time relationship because, after all, the whole of technology is built on knowledge and science is nothing but organised knowledge. I cannot see how you can put in a completely arbitrary time scale. The whole
of technological development relies on knowledge. You don't make something out of nothing. Existing ideas or new chemicals for example turn up and they are used. For instance, the principle of lasers was known for years before it was commercially exploited and now they have applications all over the place. My point is that your argument is contingent on the time you allow.

Jevons: That illustrates one of the difficulties of the Byatt/Cohen approach, according to which the curiosity-oriented discovery acts as a trigger to set off a chain of events leading to industrial innovation.

One of the studies which claimed to find a large input from basic research into innovation is the TRACES study which was carried out at the Illinois Institute of Technology. TRACES is an acronym from Technology in Retrospect and Critical Events in Science. This study took 5 innovations and traced their genealogies back. It was claimed that 70% of all the critical events were basic scientific events. TRACES was funded by the National Science Foundation, which is not an uninterested party. If you go back far enough, all innovations, I suppose, will show inputs which can be traced to Newton and Galileo.

Birch: I want to comment in a rather general fashion. The whole basis of scientific knowledge is rather like language in communication. Without language you cannot communicate; without the knowledge, for example, of molecular structures, syntheses and the work done, say, on steroids such as cholesterol, the oral contraceptives could not have been invented. I use the word ‘invented’ quite advisedly. I draw a much clearer distinction personally between science and invention than most other scientists appear to do. I regard a lot of what is called science as in fact invention and related to this ‘need’ idea. It is rather a subtle point because needs and discoveries get all mixed up.

Jevons: They get particularly mixed up in chemistry, which is your own area.

Birch: Concerning the contraceptive pill, the development of which was in one of the American TRACES studies, they had all the background wrong.
References

2 Centralism versus Pluralism in Science Policy

Who should run Australia’s scientific research?
*The Canberra Times* 19 June 1975

The basic tension underlying government support for science has been highlighted recently with the formation of the Australian Science and Technology Council (ASTEC), 11 of whose 12 members were announced a month ago, and more clearly with the furore during the past two weeks over the move to transfer some parts of CSIRO to the Department of Minerals and Energy.

Should science be run by scientists for science or by others for other purposes?

Of course, the dichotomy is not really as sharp as this simple formulation suggests. It is argued, on the one hand, that sheer excellence in science will spin off practical benefits for the rest of society. Equally, it is obvious that other purposes cannot be well served unless the science itself is good.

But the underlying tension remains: Is science to be viewed essentially as autonomous or as instrumental?

*Academic analyses*

Academic analyses of science, especially by American scholars, have suggested that scientists, if left to their own devices, would tend to do pure research rather than aim at the objectives of the wider society.

Thus the sociologist W. Hagstrom¹ sees scientists looking for rewards to their peer group — namely, fellow scientists, for whom they want to get recognition for the quality of the research papers they publish. If rewards and values from outside science intrude, the smooth operation of the knowledge-seeking system is disturbed, argues Hagstrom.

An influential analysis of scientific method and scientific knowledge by T. S. Kuhn² suggests a mechanism for the tendency to ‘purity’ in research. Science is so successful largely because scientists operate within the frameworks of established paradigms — existing
theories which provide powerful problem-solving tools and techniques.

But the more successful the paradigms are in guiding problem-solving the more they orient scientists away from socially important problems. According to Kuhn, therefore, scientists tend to tackle problems they can solve rather than problems that 'need' solving from the wider society's point of view; research is, as P. B. Medawar has put it, 'The art of the soluble'.

How far scientists behave like this in real life is a matter for debate. Many of them certainly have no desire to be isolated from the rest of society in academic ivory towers. Nevertheless, some governments have in recent years taken steps to strengthen the coupling of science to social needs.

Changes in Britain

In Britain, the Government endorsed in 1971 the 'customer-contractor principle' recommended by Lord Rothschild for applied research. 'The customer says what he wants; the contractor (researcher) does it if he can; and the customer pays'.

Here the implication is clear: the formulation of needs and priorities is not to be left to scientists and technologists. To bring more research within the scope of the customer-contractor principle, part of the funds allocated to the autonomous research councils for agricultural, medical and environmental research have been gradually transferred to the appropriate governments departments.

Predictably, there were protests from some of the scientists concerned, especially in the medical field; but others, especially among those concerned with agriculture, welcomed the changes as bringing about more down-to-earth attitudes to practical objectives.

The difficulties lie in the lack of information and imagination on the part of 'customers' for research. Scientists may all too easily conclude that 'they don’t know what they want'.

This may apply even with 'customers' who are relatively well organised and informed, such as technology-based industries. Thus attempts in Britain to define requirements for new engineering materials came up against a blank wall. Detailed surveys showed that what industry appeared to want was mainly existing materials at lower cost. Although reduction in cost of materials already available is a valid research objective, it hardly makes for an excitingly innovative program.

Nevertheless, a patronising attitude by the scientific community is
a danger that should be guarded against. There is growing resistance
to the idea of a technocratic society run according to 'scientific'
values rather than the 'general' values of the community at large.
The need, surely, is for mechanisms by which scientists can more
effectively discuss objectives with laymen rather than taking — or
appearing to take — decisions on their behalf.

For reasons analogous to those advanced by Rothschild, the
British Government has also rejected pressure by the Parliamentary
Select Committee on Science and Technology to establish a Ministry
for Research and Development whose functions would include the
examination and approval of all government research, including
defence.

Each Minister, the Government argued in 1972, must be fully
accountable for ensuring that his department's policy objectives are
properly backed by all necessary facilities, including research and
development. He could not carry this responsibility if the authority
for approval of part of these facilities were placed elsewhere, with an
independent Minister.

Moves in Australia

Similarly, the Australian Department of Science, formed by the
Labor Government immediately it came into office in December
1972, has rejected the idea of a centralised science budget as
impractical.

The White Paper which in January this year announced the
setting-up of ASTEC said that in the future, as in the past, 'the
prime carriage of specific scientific or technological subjects will lie
with the departments or statutory authorities having the major
responsibilities for them'. Where more than one department or
agency has an interest in a particular issue, inter-departmental
arrangements for co-ordination will continue.

Yet the major purpose of ASTEC was said to be the creation of a
more coherent and cohesive approach to the Government's total
commitment to science and technology. It is as yet difficult to see in
any detail how ASTEC can do much to help achieve this.

Indeed, the role of ASTEC remains altogether unclear and
undefined. Will it, as it has been suggested it should, concentrate on
long-term rather than immediate issues? Eventually, maybe, but
among its first interests are Antarctic research and marine science,
both of which are issues on which decisions are needed soon, and
which are also among the concerns of the Department of Science
itself. Perhaps, in the long run, one distinctive function of ASTEC will be to provide for wider public discussion of issues than is appropriate or possible for a government department.

The status of CSIRO

It has not helped ASTEC to get off to a good start that it was not consulted over the moves to transfer the Minerals Research Laboratories and the Solar Energy Studies Unit from the CSIRO to the Department of Minerals and Energy. These moves seem to be analogous in underlying motive to the Rothschild-induced changes in Britain, though there are differences in institutional form as well as in the abrupt way in which the process was initiated.

The CSIRO is a body run largely by scientists, a fact which may well have inspired some of the imputations that it has not always been as closely involved as it might have been with the users and potential users of its results.

There can, however, be no doubt as to the overwhelming sincerity of the desire in CSIRO to be useful to society and not merely to advance knowledge for its own sake. Major successes such as the atomic absorption spectrophotometer, self-twist spinning and many others testify to the fact that public money spent on CSIRO has not been wasted.

Whether CSIRO would have been even more productive under different organisational arrangements is one of those 'historical if' questions which can never be conclusively answered. What is certain is that CSIRO has an enviable reputation abroad as well as at home. When something is working well, there have to be strong reasons to justify drastic interference.

The case in favour of the proposed transfers has hardly been made, at least as far as the public debate is concerned. Moreover, there is a contradiction to which Dr Clive West, of the Australian National University, pointedly drew attention at the CSIRO press conference last week. Why should one principle apply to minerals and the opposite to forestry? Forestry research is being transferred from the Department of Agriculture into CSIRO.

And the ruffling of feathers among CSIRO personnel is not a negligible consideration. If there is one thing that is certain in the uncertain world of science policy it is this: that you can't get good research from resentful researchers.
References

9. C. E. West, 'From our Canberra correspondent', *Search* 6, (1975) 256-258; 310-312.
11. Reproduced with kind permission of the publishers of *The Canberra Times*. 
My title for today is the rather pretentious one, 'A Dragon or a Pussy Cat: Two Views of Human Knowledge', and the thoughts that I would like to share with you and get your reactions to were stimulated by attempts to think about the future. What might or should happen to knowledge in the future?

I am not ashamed to admit that I get very confused when I try to read the literature about the future, because — quite apart from the fact that literally anything might happen — there are such diverse views about where we want to go. For instance, William Leiss takes Francis Bacon to task for equating scientific progress with social progress. 'The effecting of all things possible' is the principal aim of Salomon's House in the Baconian fable *The New Atlantis.* 'The end of our foundation', says one of the fathers of Salomon's House, 'is the knowledge of causes . . . [and] the effecting of all things possible'. A splendid, stirring ideal for scientists to aim at! But, says Leiss, scientific progress is not the same as social progress. It is not necessarily desirable to effect all things possible. Today that hardly needs arguing, when 'all things possible' include the destruction of life on earth. Leiss's own criterion of social progress, however — the abatement of social conflict — is also beyond debate. It sounds fine, but maybe it's not compatible with other things which also sound fine, such as the enlarging of consciousness or human progress in other directions. Many of you will know the *Report from Iron Mountain* which argues that a state of permanent peace would not in fact be Utopia but would lead to stagnation in all sorts of ways. Some sociologists talk about 'the conflict theory of social progress' — no conflict, no progress.

Then again, some people want technology to produce goods and services in such copious abundance that we can afford to treat them with careless abandon and concentrate on higher things; while others, more conservationist-minded, remind us of the joke about the man falling from the top of the Empire State Building and
saying, as he passes the 20th floor, 'all right so far'. David Dickson, the science correspondent of *The Times Higher Education Supplement*, in his book *Alternative Technology*, seems surprised that liberty and equality don't necessarily go hand in hand. It seems to me that they can't go hand in hand because there is a trade-off between them. If you have more of one you will have less of the other and you have to decide what balance between them you want.

So there is a background of radical uncertainty about the direction of change and it's against that background that I want to consider the nature of that powerful agent of change — 'knowledge'. I do this with a very pragmatic purpose in mind. It seems to me vital that knowledge-handling institutions, such as universities, should consider the nature of the commodity they handle. You can't run a cut-price grocery store on the same principles as a public electricity undertaking — because groceries and electricity are two very different kinds of commodity. And similarly, knowledge-handling institutions ought to consider the nature of the knowledge they handle. The theory of knowledge, in other words, seems to me to have direct practical relevance. Epistemology is too important to be left to philosophers.

How adaptable is knowledge?

The basic question, I think, is this. How far is knowledge adaptable to our collective will? In other words, within what limits can knowledge change as societies change? Within what limits can we change knowledge as our social purposes change? That is the question which is put in picturesque terms by Paul Feyerabend of Berkeley, California, in a passage from which I take the rather cryptic title for this talk. In an article published in 1970, Feyerabend writes that he prefers an enterprise whose human character can be seen by all to one that looks objective and impervious to human actions and wishes. 'The sciences, after all, are our own creation', he says — we make them, they are constructed by us. And, he goes on, if you adopt this view, that 'changes science from a stern and demanding mistress into an attractive and yielding courtesan who tries to anticipate every wish of her lover. Of course, it is up to us to choose either a dragon or a pussy cat for our company. I do not think I need to explain my own preferences'.

Feyerabend's preference, then, is clear. He prefers pussycat knowledge, and pussycat knowledge is relativist; it is formed according to circumstances; it responds to time and place and so it
varies from time to time and from place to place, according to the particular circumstances. It's socially constructed; it is not imposed
on us by some outside authority; it is something that we ourselves collectively make.

What an attractive view of human knowledge this is! We are promised the kind of knowledge system we want. We are offered the opportunity to change it at will. For any kind of society we may in the future decide we want, we can have a knowledge system to fit.

Relativism derives a good deal of support from the work of Thomas Kuhn, which has become so very fashionable in the thirteen years since The Structure of Scientific Revolutions was published. According to Kuhn, we can see nature only through the spectacles of some paradigm or other. It's impossible to view nature just as it is, we have to view it through a conceptual framework or paradigm; and the paradigm is a cognitive commitment to one particular way of seeing the world. In the extreme interpretation, the choice between paradigms is based on taste. There are no logical standards, according to the extreme interpreters of Kuhn, by which we can decide that one paradigm is better than another paradigm. Our choice is a matter of preference or taste, a value judgement or an aesthetic judgement.

I do not wholly subscribe to this view. Although philosophers have convinced me that we can't be certain about anything in the material world — that all scientific knowledge is merely conjectural — that does not mean that objective truth doesn't come into it at all. I would say objective truth plays the role of the ideal at which we aim. Even though it is impossible ever to be certain that we have achieved it, it is still an ideal which we can strive to achieve.

This view of truth is one of the deceptively simple but brilliant insights which can be retrieved from the works of Karl Popper. In Conjectures and Refutations, first published in 1963, Popper says that 'all of us may and often do err, singly and collectively, but . . . this very idea of error and human fallibility involves another — the idea of objective truth: the standard which we may fall short of.' Popper is saying, you can't be wrong unless there is rightness as a standard from which you deviate; objective truth is the standard we may fail to attain.

This is a very salutary limitation on knowledge. Knowledge now appears as not a totally human creation. Some rather careless scholars have recently spoken of knowledge as a totally human creation. I think that is not quite right, though the trap is easy to fall into. I take natural science to be essentially the invention of
regularities which more or less fit nature. Scientific knowledge, therefore, is bilaterally determined. The invention must be a human act — nobody but a human being can formulate regularities; but the fit depends also on what is given externally to us — the fit depends on what nature happens to be like. And that, incidentally, seems to me to provide a place for facts in a world of values. Only a few years ago people were saying we need to find a place for values in a world of facts, but the problem now seems to be quite the other way round. All facts have been dissolving in a sea of values and the values are changing all the time, since you can always change your mind about values. The notion of objective truth, at least as an ideal to be aimed at, does provide some permanence in this sea of change.

Let me now apply this very simple idea of truth to two current debates: one about alternative knowledge systems, and the other about the autonomy of technology. I will take alternative knowledge systems first.

**Alternative knowledge systems**

There is an idea around, fashionable amongst the young, amongst my students at least, that defects in modern society are due not to misuse of science but to the nature of the science itself; that it's not a matter of improperly using the science we have, but that it's the very nature of the science that's wrong, that we've got the wrong kind of knowledge. For instance, Herbert Marcuse and William Leiss suggest that the urge to conquer and exploit nature extends itself almost inevitably into the urge to conquer and exploit man: if you like, the Baconian urge extends itself into the Faustian urge. According to this view, Western scientific knowledge is intrinsically exploitative, whether it's exploiting nature or exploiting other men. That connection may not be a necessary connection — it seems to me that the wage slaves of modern industrial society are not necessarily more exploited than were the domestic slaves of the ancient civilizations. But some people do see a link, and since they want to end the domination of man by man, they look for less domineering, less exploitative knowledge systems. That makes them turn away from the Western scientific-technological mode to Taoism or Zen or other oriental philosophers.

I'm a bit sceptical about this because I suspect that their real objection is not to the fact of the domination of nature but to the crudity and messiness of the means of domination which have been adopted so far. It is all very well to talk about living in harmony with
nature instead of dominating it — but does that mean a policy of non-intervention towards the typhoid and cholera organisms? I think not. There seems to be a basic philosophical confusion here. The desire for subtler and less obtrusive means of control is fine, but it is being confused with the suggestion that we don't want any means of control at all, which is stupid. Persuasion may be a better means of control than command, but helplessness surely cannot be a virtue in itself. The idea that technology is *intrinsically* bad is, I think, intrinsically absurd. What is bad is bad technology.

If you regard science as the invention of regularities that fit nature, science seems to me to have a monopoly which is quite unchallengeable. There is a good deal of interest now in witchcraft as an alternative knowledge system. At the annual meeting of the British Association at Stirling in Scotland last summer, one session that was absolutely crowded out was that on witchcraft. The people who went did not get what many of them perhaps expected to get. There was no sensational stories about witches dancing around on the blasted heath at midnight, but there was sociology, some of it rather turgid. Nevertheless, the drawing power was symptomatic and the issue of *New Scientist* that reported the meeting featured witchcraft on the front cover.

But how strong is the claim of witchcraft to be a genuine alternative knowledge system? If it turns out that some practice derived from African witchcraft cures disease, it could perhaps be explained, so some anthropologists suggest (Robin Horton, for instance, at Ife in Nigeria) by the diagnosis and the alleviation of social stresses at which the witchdoctor may be very adept. But that practice then becomes an alternative *within* science, because it works: it is a regularity in nature. It's not an alternative *to* science, it's an alternative *within* science. It's not an alternative *to* science any more than a new source of energy would be. A new source of energy would increase the repertoire and the range of natural science, not provide an alternative to it. The current repertoire of science is obviously imperfect and incomplete and needs extending in all sorts of directions, but I don't think it can just be replaced.

**The autonomy of technology**

Let me move on now to the question of the autonomy of technology, which turns out to be an issue more similar to that of alternative knowledge systems than might at first appear. It's fashionable in some quarters to insist that technology is not autonomous but is
socially determined, or, if you like, politically determined. This insistence is a reaction against the idea that technology is some irresistible force beyond our control which is running away with us. Seymour Melman argues against what he calls 'The myth of autonomous technology'. So does David Dickson, in the book to which I referred a few minutes ago called Alternative Technology, published last year. Much of that book is devoted to an attack on what he calls 'the myth of the political neutrality of technology'. Dickson says technology is not politically neutral or autonomous but socially determined, under our control: it's a political matter what kind of technology we go for.

It's true of course that social preferences influence technology and the way in which technology develops. What we have decided to do in the past in war, in transport, in space and so on has profoundly affected the kinds of technology that we have. If we had made different decisions, we would now have different technological capabilities. But the match between our needs and the technology to meet those needs is never complete or perfect. If it were complete and perfect, technology would be the ideal slave — the sort of slave that does exactly what we ask it to do and nothing else besides, like a servant who never asks for any time off. Such an ideal slave unfortunately does not exist. Melman says that each machine has built into it 'the particular requirement of whoever decides its characteristics and the uses it must serve'. That's true, but the characteristics are not exhaustively described by the user's specifications. Melman says, again, that it's 'a distortion of understanding' to make technology 'appear as though independent of man's will'. That's true, but it's equally a distortion of understanding to deny that it is partly independent of man's will.

The social determinist view of technology, it seems to me, is a misleading half-truth because it ignores two things. First, it ignores those effects of technology which were not foreseen or not desired or both. The effects of DDT on bird populations were presumably not politically determined; nobody wanted them — they just happened, against our will. Nor can I see it as part of the social function of the development of transistors to force pop music on those who don't want to listen to pop music.

Second, the social determinist view of technology is based on an inadequate appreciation of technology as a range of capabilities. That range goes beyond what is in use at any one particular time. One of the examples David Dickson uses is the telephone system. The system as we have it, he says, reflects the individualistic nature
of our society because it's designed for communication between two individuals, one at each end, rather than for group discussion. But to adapt the telephone for group use is easy. We can already have limited conference facilities by telephone. The technology is virtually identical — only small changes are needed to get group discussions by telephone. The way we actually use the technology is of course determined by social preferences, but the range of capabilities that is inherent in the technology is not determined by them.

Let me take another example of a similar kind. The 'stabilised world' of *The Limits to Growth* by Meadows et al. at the Massachusetts Institute of Technology is stabilised only with lots of recycling, pollution control, soil restoration and so on. The argument for zero growth is really far from being an anti-technology argument, though some people have wrongly interpreted it that way. The argument is that technology by itself is not enough and we have to do something else besides. To get stability certain value changes are necessary as well as lots of recycling, pollution control, soil restoration and so on. Now what kind of technology would it be in that stabilised world of Meadows et al.? The particular devices would undoubtedly be different but unavoidably they would rely on largely the same principles and components and skills as the technology that we have today. We wouldn't have totally different kinds of machines, we would be using the same kinds of basic technology. We would still use wheels, and sulphuric acid, and so on. Much the same underlying know-how and hardware can serve both the god of growth and the fad for zero growth.

I conclude that technology *does* have a substantial degree of autonomy. In some important sense it is politically neutral and culture-independent; it can transcend social change. It is not determined entirely by society but it has some life of its own, some properties which we don't intentionally put into it. In short, technology is not created entirely according to our wishes.

Some practical implications

The common feature that I have tried to draw out of these two issues, the issue about alternative knowledge systems and the one about the autonomy of technology, is the recognition of some constraints on our knowledge systems. We can't mould knowledge entirely to our own tastes, because of what is given, what is there external to us and independent of us. This is a significant qualifications which some commentators in recent years seem to
have overlooked. In Feyerabend’s terms, knowledge has to be recognised to have a dragon element as well as a pussycat element. I am not very upset by that recognition. Personally I’m not fond of cats anyway and I think that dragons could be rather useful if only we can get them on our side. The problem is how to make sure that the dragons are on our side, fighting with us and not against us.

To some extent, knowledge dictates to us how it has to be handled. That’s one of the dragon-like characteristics of knowledge that is beyond our control. And the most important implication, it seems to me, is that students are well advised to put up with some degree of dogmatism from their teachers because it’s by putting up with that degree of dogmatism that they are able to draw on the store of accumulated experience which helps to give them expertise.

The notion of accumulated experience inherent in scientific knowledge can be found in the epistemologies of both Popper and Kuhn, though in rather different forms and in each case in a rather backhanded sort of way. For Popper, the supreme commandment is to be critical. The good Popperian scientist is always critical — he is always trying to overthrow existing theories. Nevertheless, the current corpus of scientific knowledge is the result of a long process of evolution — a long trial and error process of learning from mistakes, analogous to the evolution of living organisms. The sub-title of Popper’s most recent book Objective Knowledge,15 published in 1972, is an evolutionary approach. The theories which have been refuted in the past correspond to evolutionary mistakes. Modern knowledge embodies hard-won experience just as modern living organisms embody a long series of evolutionary mistakes; at least, they embody those mistakes in the negative sense that they are not there, having been eliminated.

In the case of Kuhn’s epistemology, although it’s questionable whether the succession of paradigms is cumulative — whether one paradigm really is ‘better’ than another — it is nevertheless clear that, within a paradigm, problem-solving power is acquired by accepting its ground rules. Unless you accept the ground rules which the paradigm defines and the mode of approach which it suggests, you don’t acquire the problem-solving power which is inherent in that paradigm. ‘A more efficient mode of scientific practice begins’, says Kuhn, when a group of workers ‘take the foundations of their field for granted’.

If there is anything in this view, education in science must have something of the character of an initiation into a problem-solving tradition. A dogmatic element is an epistemological necessity. The
epistemological price that students have to pay is the recognition that their active participation cannot extend to the creation and the criticism of all the theories and the conceptual frameworks that they use in their science. and the consequence of that for university practice is that student-centredness, wonderfully heady slogan though it is, has to be tempered with subject-centredness, because there are some things about which teachers do on average know better than students. Why is it that we feel that if universities didn’t exist we would need to invent them? The answer surely must include the need to make the experience of the past available to the present for the benefit of the future. I expect you all know the old joke that the function of education is to let the young learn from other people’s mistakes instead of having to make their own. The joke is really quite profound. Education boils down to just that, when viewed in the light of Popper’s theory of knowledge.

There are also implications for the power structure of universities. Radical sociologists of education, notably Michael Young in London, are emphasising that the curriculum in schools and universities is socially determined. What counts as valid educational knowledge is socially defined by those who hold power in the educational system — in short, by the politics of the curriculum. There is an element of truth in this, but it is only half of the truth because the reverse also holds: the distribution of power reflects the nature of the knowledge that is being handled, or at least it should reflect the nature of the knowledge, and unless it does, that knowledge cannot be efficiently handled.

Perhaps that sounds very conservative because, clearly, it is an argument in favour of power for the teachers rather than for the students. Is this appeal to accumulated experience just the time-honoured reactionary cry which has so often been raised by academics to resist innovation? Is it just a slightly new guise for the old argument that ‘we, your elders, know better, therefore you cannot have power in the educational system’? I think it’s very far from being just the time-honoured reactionary cry, for two reasons.

First, it is not an argument for no change: rather it’s an argument about the type of change which is required. Change, I suggest, should be a matter of redirecting existing knowledge more than finding substitutes for it. Those are two very different matters. A different strategy is called for if you are redeploying knowledge rather than replacing knowledge. It is replacement which is implied by the call for new knowledge systems and the fascination with Taoism and Zen instead of Western science. Over-emphasis on
replacement might, however, turn out to be counter-productive because it might reduce the stock of what is available for redeployment. To the old question of whether the established disciplines are necessary, I think the answer is probably yes, they are necessary, but they have to be used flexibly. Many people have tried to mount ‘environmental’ courses of one kind or another and the more heady revolutionaries have claimed to be developing new knowledge systems in the process. But when you get down to looking at the knowledge that is used, it is mostly old knowledge redeployed to meet new sets of problems.

Second, I am not suggesting that all knowledge is exclusively of the dragon type. I have chosen today to emphasise the dragons, but that doesn’t mean that we can forget all about the pussycats. They also exist. There are aspects of knowledge where continual recreation is just as important as drawing on the stock of accumulated experience. In these areas, students can learn as much from each other as from their teachers. When they are dealing with this kind of material, teachers might do well to adopt the fellow student role and make sure their students can occasionally beat them in an argument.

Understandably, this kind of knowledge is increasingly popular amongst students. Pussycats have a more immediate appeal than dragons. It would be all too easy, as education expands, to leave things increasingly to the pussycats rather than the dragons. After all, a relatively small number of engineers and technocrats would be enough to keep the wheels turning in the society of the future. A few such people can service the railways and the airlines and so on, leaving the rest of use free to indulge in aesthetics or athletics or whatever takes our fancy.

Far be it from me to say that we should force all students into the technocratic rat-race. Most certainly I don’t want to do that. I do, however, fear that unless we are careful we might end up with an undesirable distribution of expertise through society. And that is why I have chosen to emphasise the dragons today. I feel we should avoid that I’d like to call ‘cliff effects’ in the distribution of knowledge — that is, abrupt discontinuities, with some people having all the expertise and others having none. For experts, isolated in their islands of expertise, it’s all too easy to suppose the choices are technologically dictated. It would not be surprising if nuclear engineers regard the desirability of nuclear power stations as obvious. And that leads to the sort of one-dimensionality that Marcuse complains of, where options become disguised as imperatives: what should be free choices come to look like forced
ones. I would like to see the contours of expertise widely spaced so that there is plenty of overlap between the fields of competence of different individuals. Expertise should not be exclusively concentrated in islands but distributed in overlapping fields of competence. I don’t see how we can otherwise maintain a genuinely pluralistic society, with a range of interests genuinely represented in setting social goals and with expertise made and kept genuinely accountable to the wider public. It is for this reason amongst others that I hope the dragons won’t be forgotten as education expands.17

Discussion

Questioner: Early in your talk you made the observation that you thought that the division of knowledge into disciplines ought to be maintained. Yet later you said that there ought to be a wider distribution of knowledge. Could I interpret that to mean that you believe that knowledge-handling institutions should be restructured so that people are not necessarily funnelled into particular disciplines at an early age but that they should get a wider appreciation of a number of disciplines?

Jevons: That is a difficult one. I have been in the business of wider education for 9 years now, so I almost have a vested interest in breadth. Nevertheless, I think that you can’t easily get away from the existing disciplines. Chemistry is there and there is a limit to the extent to which you can devise an alternative chemistry. Some of the people plugging ‘environmental’ courses have suggested that one of the reasons that many academics resist them is their fear of an area where they don’t know more than their students, so that their power and their status are threatened. However, I think that argument is back to front. When a student of an environmental science course comes up against a problem which requires some chemistry — for example, when he has to determine the concentration of sulphur dioxide in the atmosphere — he is all the more at the mercy of the skilled chemist because he knows hardly any chemistry himself. That illustrates one limitation on attempts to scan the whole field of knowledge all at once. In Britain, the indications are that there is almost an overproduction of ‘environmentalists’. The authorities are saying that young people should start off in one of the better established disciplines and then branch out into environmental courses, pollution control courses, etc. It may be that the authorities
are being too conservative, but that is the attitude they have expressed.

*Questioner:* I don’t see how that gets over what you were saying right at the end about a wide spectrum of possibilities.

**Jevons:** My argument at the end was for people who combine within themselves limited expertise in more than one area. One of the standard things to do when you have an interdisciplinary problem is to bring together people trained in single disciplines — say a chemist, a sociologist, an economist and a psychologist. However, as long as those people each remain exclusively specialists in their own disciplines, there cannot be overlap or fruitful interaction. Even more important is what happens on the grand scale. How can a society handle science if it leaves the management of its industry and the administration of its government entirely to people who have studied only administration, economics and so on? Can such people make the best use of the science and engineering that goes on in society? I would suggest that we need some people who have some scientific background but who have gone into administration. That is an argument for a wider form of education.

*Questioner:* Are you suggesting that all generalist administrators should have one area in which they are expert?

**Jevons:** I don’t see that as necessary. The argument about the right kind of education for administrators has gone on for years and I think it has no unique answer. The people who select Civil Servants in Britain are at pains to emphasise that they don’t mind what subject has been studied by the applicants. The qualities they are looking for are independent of the subjects studied. I can say this from first hand experience because I have sat on the selection boards. However, it would be a pity if within the whole structure of the administrative Civil Service that there was nobody who had some sort of scientific and technological background. For the purely arts-trained man, science and technology seem rather daunting. Some scientific background helps to break the ice and to give confidence in tackling issues which have a technical content.

**West:** Is it true to say that the two cultures are less separate now than when Snow first put the idea forward?
Jevons: Yes, I believe that is the case in England. I wish it were more the case than it is. The boundary is only just beginning to get fuzzy and I think it should get very fuzzy indeed.

West: Yet, some people say that there are really three different cultures with the interdisciplinary people in the middle unable to relate to people at either end. Perhaps that is another argument.

References

2. F. R. Jevons, 'But some kinds of knowledge are more equal than others', *Studies in Science Education* 2(1975) 165-173.
4 Liberal Studies in Science at Manchester

Lecture delivered 9 July 1975

What I want to do today is to share with you some of my experiences in that humble but interesting trade which most of us are in — that is, the business of university teaching and research. Many of the problems are general ones and I would like to hear how you feel about them.

The Department of Liberal Studies in Science was started at Manchester University in 1966 with the purpose initially of running an undergraduate course. The idea was that a broader science-based education should be provided to facilitate the movement of people with a scientific background into management and administration and a whole range of jobs much wider than the outlet for which traditional scientific education is mainly designed, that is, scientific research. Looking back now on that formulation of the aim, it is perhaps characteristic of the mid-1960s. It was then rather widely assumed that what society really needs is more scientists in position of managerial and administrative authority: if only we could get more scientifically educated people running the whole show, the problems of society would soon be solved. That sort of assumption is much less widely made now; it's less widely agreed that the more scientists we have moving into administration and management, the better society will be. Nevertheless, I don't see a need radically to change the formulation of our aims. Science and technology are still valuable resources. Fortunately they are not non-renewable resources, but quite on the contrary, they are resources that we can renew and develop in ways that are partially under our control. They can help to achieve a wide variety of social objectives. Perhaps it's all the more relevant now to look harder at how we use them. If there is some disillusion with science, if science is no longer considered to be by definition a good thing, if it's good only if it's well used, then there is all the more need to be carefully selective about what we do with science. (I am using the word science broadly now to include technology.)
With this in mind, we need to avoid any sharp separation between specialists and laymen. We need (to put it in the way I formulated at the end of my last lecture) networks of overlapping fields of competence so that people have enough in common to talk to each other. Educationally, I interpret this not only in terms of specific kinds of knowledge but also, and more, in terms of basic skills. To define the basic skills the very simple classification into literacy and numeracy is still useful. Rudimentary though it is as a taxonomy of educational objectives, I think it is still useful. A primary aim of the undergraduate course in Liberal Studies in Science is to develop literacy as well as numeracy. Literacy means principally the skills of extensive reading (rather than the intensive reading of textbooks, which the physics or chemistry student typically does), selecting relevant facts, marshalling those facts to develop arguments in extended prose, defending one's position in essays and seminars and so on. Those are the basic skills of literacy considered as a communication skill.

It is not true, as has sometimes been suggested, that scientists and technologists are by and large illiterate. Some of them are very highly literate, and some of the very best English I know has been written by scientists. But it is true that, if scientists and technologists are not illiterate, they don't owe that to any large extent to the kind of higher education most of them have received. They have developed their literacy more despite than because of their education at university and, in England at least, in the sixth form as well.

Origin

Perhaps you would like to know a bit about how Liberal Studies in Science arose. My sociologist friends tell me that innovations rarely come from the middle ranks of seniority but tend to arise from the top or the bottom, from the most senior people or the most junior people. And the most radical innovations tend to come from the bottom. Liberal Studies in Science at Manchester fits that generalisation. It was started from the top. The initiative was taken by Professor Flowers, now Sir Brian Flowers: he was then our senior Professor of Physics, later became Chairman of the Science Research Council and is now Rector (that is, effectively Vice-Chancellor) of Imperial College, London. One of the chemistry professors, Professor Gee, a skilful, unflappable committee man, did much to see Flower's brainchild through the committee process. Flowers was even at that time active in science policy circles in
Britain, and he was struck with the difficulties that civil servants sometimes found themselves in. The British Civil Service, like the Australian Public Service, has a lot of harsh things said about it but in fact it contains many able and devoted people. When, however, they come up against issues with a technical or scientific content they seemed to Flowers to be at a disadvantage because most of them have studied no science since they were about fifteen. It was to overcome this kind of difficulty that he wanted a broader kind of science education.

So the origin of Liberal Studies in Science was from the very top, from the senior professors of physics and chemistry. And the innovation was not too radical; it was not the really radical sort of innovation which tends to arise from the bottom. It was quite a departure in the context of the mid-1960s, and was recognised as such by the educational world and by the press (we got a fair coverage in the press at the time) but we have not set out to turn everything upside down, and I will tell you in a few minutes about some ways in which we are relatively traditional.

Staff

How did we recruit our staff? I was the first to be appointed. At the time I was in the Department of Biological Chemistry at Manchester; I had written a few papers on the history of science, and I was getting interested in the more modern problems of the role which science plays or could play in society. When the chair was advertised I applied and I got it. All I can say in self-justification of that appointment is that if you never let people do things for which they are not adequately prepared, a lot of worth-while innovations will never take place. I feel rather the same way at the moment about the Vice-Chancellorship of Deakin University.

All the staff that we have recruited so far have been ‘switched’ scientists; that is, they have been scientists in the sense that they have taken science or technology at least up to first degree level. Although we have never ruled out the possibility of recruiting staff from other backgrounds, we have not yet done so. If we had been setting up a multi-disciplinary research unit to study problems of science policy or science in society, we should certainly have recruited social scientists as well as natural scientists. But our situation was not quite like that. We were in the middle of a large university which had rather big and rather strong social science departments. For inputs of economics or sociology or political
science, we preferred to draw on those departments, and that enabled us to use the small number of our own staff appointments for 'switched' scientists.

In the early years it had to be good enough for a scientist applying for a post to express good intentions about switching; but now, before we appoint someone, we can ask for some sort of demonstrated achievement in the new field. That is a sign of the field becoming somewhat more established and institutionalised. When we made three appointments a year ago (two replacements and one additional), all the appointees had already published a few papers in the field.

Another change from those early years is that in those times we had one man-year in hand, so to speak; that is, we were able to appoint a Lecturer one year before we actually had to ask him to give a course. But now, with the growth in our student numbers, we have a rather bad staff/student ratio. The staff expansion now has to be student-led; that is, we have to have the students first and use student numbers as our argument for getting more staff members. We have just managed to get authorisation for a new staff appointment, bringing our total size to eight members of teaching staff. That does not seem much by Australian standards of expansion but by British standards it is not bad for a subject which many people considered to be a non-subject when we started. The financial climate in England now is even chillier than in Australia, and many departments are pleased if they can just get authorisation to replace members of staff when they resign.

Undergraduate course

Let me briefly outline for you the plan of our undergraduate course. In England the standard pattern is a three year, not a four year course, and nearly all the students are honours students. The general plan for our students is that they spend about half their time studying science itself and about half their time studying Liberal Studies in Science; there is room also for optional subsidiaries in the first and second years and for a thesis in the third year. I will take these three elements separately.

The science itself now consists of three separate streams of which each student chooses one. Roughly speaking, they are physics, chemistry and biology respectively. The content is not strictly confined to those subjects, of course, but there is a physics-based stream, a chemistry-based stream and a life science stream. The life
science stream started only last October and, with that addition, our total undergraduate intake went up to forty-four. Previously it had plateaued at about thirty; that is, about thirty students were coming in per year to take the physics and chemistry streams.

The tendency has always been, every since we first started, for the students to want to decrease the science component. They have always been more interested in talking about science than in actually doing some science. I have always resisted this tendency because it seems to me that the whole purpose of the exercise will be lost if they don’t get some genuine science taught by genuine scientists. Science is taught not by us but by the science departments — mainly physics, engineering, chemistry and the life sciences, with contributions also from computer science, geology and so on. Some people have occasionally suggested we should do our own science teaching but I have resisted that suggestion. Although it might be an attractive solution in the short term to the problems of getting presentation of science specially tailored for non-specialist students, in the long term there could be disadvantages in having science taught by scientists cut off from their parent disciplines.

In the subject called ‘Liberal Studies in Science’ we deal with the social relations of science. We get contributions from Economic History and from Economics, and in the past we have had contributions from the Department of Government; but we teach most of this material ourselves. My earlier seminars have given you some examples of the sort of topics that we deal with. It has been important for us that we have an identifiable subject area of our own, because that has enabled us to have a Department — a Department which can act as a base in the University for our undergraduates and which gives them and us an identity. I think this is a real advantage that we have over those broad courses which are constructed by merely mixing existing subjects. Many universities have tried to construct broad courses by putting together a bit of this and a bit of that, but the students tend to lack an identity and a home in the University. Fortunately we have been able to provide that. Although we do only about one third of the total teaching that our honour students receive, averaged over the three years, they feel from the very beginning that they are our students. We interview them for admission, we admit them, we register them in the first week of term when they come up, and they know and we know that they are our students.

We have a tutorial system based on the idea of one principal tutor for the year. That tutor has always been one of the members of staff
who teaches a course in that year. The alternative would be to have a ‘moral’ tutor who does not teach them, separating the academic and the ‘moral’ functions. We have always felt that the advantages of that system are outweighed by the advantages of having as tutor somebody who regularly see the students anyway in an uncontrived situation. In teaching his course he sees them once a week at least in a seminar or tutorial to discuss essays or something similar and that provides the sort of contact from which other tutorial guidances can spring.

As a piece of self-indulgence I have always, except for one break of two years, taught a first year course myself. This means I have the edge on my colleagues because I can get to know all my new students by name within their first couple of weeks at university.

Let me tell you now about one relative failure. I am not sure how serious a failure it is but certainly it is something which we tried and which we have not pulled off. When we started in 1966 it was intended that physicists and chemists should get together to teach an integrated physical science course to our students. We worked hard at that in the early years but a few years ago we gave it up and there are now two separate streams, as I said a few minutes ago, physics and chemistry. The reasons why we failed to integrate physics and chemistry to produce physical science are not entirely clear to me. It is possible that physics and chemistry are incompatible, that they are like oil and water and that they just don’t mix; for instance, electromagnetism and organic chemistry may just be so very different that there is no way to integrate them. But what slight indications I have about the reasons for the difficulty suggest rather the opposite. The difficulty seems to arise not because of the differences but because of the similarities between physics and chemistry. It is over areas like thermodynamics that the physicists and chemists really disagree. They both have a stake in thermodynamics but they take rather different views of it and find it difficult to agree on how to approach it. That is a very tentative explanation for our failure to produce an integrated physical science course.

Interdisciplinarity is of course the new orthodoxy. You have probably heard the story of the trendy university in which it was decreed that henceforth all disciplines shall be interdisciplines. In our time we were considered to be in the vanguard of the interdisciplinary movement but my more mature reflections on interdisciplinarity suggest that it can be overdone. I no longer believe that the disciplines are such barriers to progress that they need to be abolished. I think that the established disciplines need to
be redeployed rather than replaced. Disciplines like physics and chemistry and biology are valuable and tested modes of approach (Chapter 3). They are amongst the most successful knowledge systems that mankind has ever devised. It seems to me sheer lunacy to throw them overboard. What should be done is to redeploy them, to use them in different combinations and in different ways, and to focus them on different problems.

Moreover, I think you need substantial chunks of them. Physics and chemistry are not the sorts of subjects which lend themselves to infinite subdivision into minute little pieces. I like to compare the undergraduate curriculum with a salad. Salads are popular because they are varied mixtures but one likes to find recognisable chunks of lettuce and tomato in them; one doesn't want to find a homogenised paste where no ingredient is recognisable. Homogenised pastes are baby foods. Correspondingly, in the educational sphere, the fully integrated day may have virtues in primary schools. At the undergraduate level, however, intellectual palates are more sophisticated and I think they like to have recognisable chunks of primary foods, namely physics, chemistry and so on. Possibly this view is too conservative. Certainly some people in England to whom I put it think it is reactionary — just an excuse for not trying harder to find new ways of presenting material. It could be that we should be trying harder to teach a new physics or a new chemistry. I have to leave this question as an open one.

Theses get mass-produced in my Department. Every honours student does an undergraduate thesis in his third year; and on top of that there are the M.Sc. theses, to which I will refer in a minute. We have only a small staff of our own because much of the teaching of our undergraduates is done by other departments, so the supervision of theses is a real problem. We have tried getting supervisors from outside the Department, but that has never really worked, so there is a very high thesis to staff ratio. The topics range very widely. We don't allow our undergraduates to do quite anything — it has to be something to do with science — but that still leaves a wide field. We have had well over a hundred theses by now, and the topics include most of the possible pollutants that you can think of, and most of the possible energy sources, and most of the non-renewable resources. There are historical ones such as ‘The Royal Manchester Institution: 1823-1881’, part of which, incidentally, has since been published; there are political ones at home such as ‘The Cabinet Decision to Close the Tracked Hovercraft Project’; there are political ones abroad such as ‘Technological Innovation in the Soviet Union’, there
are some dealing with technological issues in less developed countries such as China and North Korea; there are literary ones such as ‘Arthur Koestler’s Views on Science’; and there are ‘alternative’ ones on matters such as extrasensory perception, Taoism, and so on. With such a very wide range of topics, it becomes a real strain on us as staff members to provide helpful supervision for our students. Theses always seem a wonderful idea when one is planning courses, and it is true that it is a tremendous stimulus during planning to talk about the possibilities for thesis work; but in the event one can have too much of a good thing.

Jobs

To the perennial question ‘what sort of jobs will these graduates get?’ there is no specific answer because the sort of education we are providing is not designed for specific job slots. Our graduates tend to go into the kind of jobs that are advertised as ‘for graduates of any discipline’. In the competition for such jobs they do rather better than average. Of course the competition varies from year to year, the main determinant being the general state of the employment market. In the early 1970s, when ‘jobless graduates’ were in the news, when graduates were finding it hard to get jobs, the Careers Centre told us that ours were amongst the most saleable graduates in the University. It is not the case that all our graduates always get the best jobs, but on average they do quite well. Perhaps the main advantage of our kind of education over the more traditional single-discipline kind lies in their own definition of their job prospects. They tend to regard a wider range of jobs as suitable for themselves. And perhaps they have somewhat more confidence in those areas that have a technical content than the straight arts or social science graduates with whom they might be competing.

Most of them, over half of our graduates, have got jobs in industry or commerce, broadly interpreted. If those jobs are research jobs, the research is not usually of the laboratory kind but in areas like market research or operations research. Few of our graduates will be wearing white lab coats at the moment. Other job titles include ‘Graduate Trainee’, ‘Administrative Services’, ‘Production Management’, ‘Marketing Economics’, ‘Data Processing’, ‘Systems Programming’ and so on. And the employers include many of the well known names in British industry such as I.B.M., I.C.L., Kodak, Mars, Mullard, Atomic Energy Authority, British Oxygen and so on. A few have gone into technical writing, which is one field where I
think our course does have quite specific advantages. One of our first batch of graduates in 1969, Colin Norman, is now one of the Washington correspondents of *Nature*. A substantial proportion of our graduates, about twenty per cent, go into school teaching. Some go on to take Master courses in a variety of areas like social administration, or operations research, and a fair number go on to Ph.D. research. It’s not true, as some people have thought, that a broad first degree cuts students off from the possibility of Ph.D. research. We don’t encourage our students to stay with us, feeling that it is often better to go on somewhere else after three years with us. Other departments who are prepared to have our graduates to work for Ph.Ds are not as rare as you might think. They include departments with titles such as History and Sociology of Science, Science Education and so on.

**Postgraduate course**

Although we started with an undergraduate course, our clearest teaching success is our M.Sc. course, which began in 1969. It has been very successful in the sense that we have got plenty of good applicants and we get generous support with studentships from a joint committee of the Science and Social Science Research Councils. It was originally intended for those with degrees in science or technology or with degrees including science or technology, but in the last few years we have taken a few people with arts or social science degrees and I am now particularly keen to encourage that. This means some change of definition of the purpose of the course. Whereas originally it was mainly to broaden the education of scientists who felt that their first degrees were rather narrow, I would like now to draw in more social scientists and arts people to ‘enculturate’ them, as far as is possible in one year, in the culture of science. In the medium term this may become important. There may soon be shortages of physical scientists and engineers because of the small numbers taking physical sciences in the schools. If there are shortages, those who do qualify in these areas will be wanted for specialist jobs and people to play interface roles will have to be drawn from the social sciences and arts.

The M.Sc. programme does not contain any natural science as such; its content corresponds roughly to the Liberal Studies in Science component of the undergraduate course. The title of the programme is ‘The Structure and Organisation of Science and Technology’. It is somewhat more vocationally oriented than the under-
graduate course, in that we have an input from the Manchester Business School, one of the two national business schools in Britain, which has a special unit called the R & D Research Unit to do 'research on research'. We have very close contacts with that unit. We have provided them with many of their research personnel and they in turn provide us with some teaching.

We started the M.Sc. course after we had run the undergraduate course through once, by which time we felt we were ready to go on to a postgraduate course. If I were beginning again now, though, I think I would start with the postgraduate course. It would probably be easier — there are fewer resistances to starting a new subject at the postgraduate level. It seems less of a threat to other people and it's less permanent; one could close down the course at one year's notice, whereas at undergraduate level there are three intakes of students in the system at any one time.

This year we have twenty-five students, which is a large M.Sc. class by British standards. Some of them are post-experience — about one-third have had some sort of experience since graduating. We are very glad to have such people participating because they provide more mature inputs during the seminar discussions. Some come from polytechnics or colleges of education, using us for 'in service' training, a few are seconded from Government science policy bodies, for instance from Indonesia and Sri Lanka. We have had several Canadians and this year we have our first Australian, a graduate of the University of New South Wales. I hope we will have some more.

SISCON

My department belongs to the SISCON scheme. The acronym SISCON stands for Science in a Social Context, a Nuffield-financed collaborative scheme which brings together seven universities and one polytechnic in Britain to prepare and test learning and teaching material on science in a social context. The idea originally was to prepare material which students would read and then come together to discuss with a staff member. There has, however, been a change of emphasis during the two or three years since SISCON started because it has now become apparent that the people most in need of help are not the students but potential teachers. Physicists or chemists who are relatively underemployed often want to switch to teach this new kind of material and they need guidance about what to teach and how to teach it.
Testing such material is a big job, as I can now testify from personal experience. In my first year course I tried out one of the sociology of science units and as a result I now appreciate more fully the difficulties that can arise and the size of the commitment necessary to set aside in advance a substantial part of an undergraduate course to test new learning and teaching material. SISCON would like to have more people testing materials. The contact is Dr W. F. Williams, of Leeds University, who is the co-ordinator of the scheme.

Research

Let me finally say a little about our research programme. We have got a large postgraduate population: in addition to the twenty-five M.Sc. students, there are eighteen Ph.D. students, making us the fifth largest postgraduate school in the Faculty of Science — only Maths, Physics, Chemistry and Computer Science are bigger. There are also Fellows and visitors, about half a dozen at any one time.

Our interests are wider than just science policy in the strict sense, if you take science policy in the strict sense to refer to governmental decisions affecting or involving science. If I had to define our interests now I think I would say something like this: that we want to explore the interface between the natural and the social sciences, where the natural and the social sciences interact. That is a pretty wide field. Our closest counterparts elsewhere in Britain are at Sussex and Edinburgh but about a dozen other universities are involved in one way or another. All have different emphases: the field is not yet clearly defined.

In our current list of research in progress, thirty-three projects are listed. That includes Ph.D. but not M.Sc. projects. You have to allow for a certain amount of exaggeration for public relations purposes and a certain amount of claim-staking by my colleagues on the staff, but even when those allowances have been made, a great deal remains. My Department is quite a lively place at the moment. I have been very lucky in the colleagues that have come to work with me there.

Let me tell you about a few of the things that are going on. We are maintaining our interest in industrial innovation. There is one study on innovation in the pharmaceutical industry, which is really an attempt to extend to the life science area the sort of investigations that I talked about in my first lecture, trying to identify the kinds of inputs that come from basic scientific research to industrial
innovation. Our earlier work dealt largely with the physical sciences relating to the engineering, chemical and allied industries. This project is an attempt to see how the situation differs in the relationship between the life sciences and the pharmaceutical industries.

We are also trying to study the responses of industry to changing resource availability and we have picked copper and mercury as the particular resources to study. The *The Limits to Growth* study (the Club of Rome-backed study of Meadows *et al.* at the Massachusetts Institute of Technology) said we are soon going to run out of resources. According to the listing in that book, made five years ago in 1970, we should already be well on the way to exhaustion in the case of several metals. We wanted to see how technology is responding to this threat. What is industry doing about it? It turned out that when you ask industrialists, they don't seem worried. Nor is the price mechanism responding. Just as we were starting the study the price of mercury plummeted downwards whereas, if it had been approaching exhaustion, it should have been reaching the ceiling. Copper prices went down quite markedly about a year ago, due to some quirk of the London Metal Exchange.

We were particularly interested in innovation responses. Are there new technological innovations coming up to conserve copper or mercury? There have been innovations to conserve mercury but they have been more in response to pollution scares than to resource scarcity. Innovations to improve mercury recovery from effluent gas and from effluent sludge have been stimulated by environmental pressures. Moreover, the price mechanism seems inadequate to look after the mercury situation. The main use of mercury is in the electrolytic production of chlorine in which the cost of the mercury is a small proportion of the total cost. The main cost of producing chlorine is the cost of electric power, so as far as the chlorine manufacturer is concerned, a doubling or a halving of the price of mercury doesn't make much difference.

In the case of copper there has not been much technological innovation. Some new processes have been developed for better recovery of copper from scrap but the main responses seem to have taken place in the structure of industry. There has been a fair amount of vertical integration — manufacturers of copper products integrating backwards with the copper producers to ensure greater security of supply.

Some people in the Department are interested in technology assessment. My trouble over technology assessment is that I am still
uncertain exactly what the expression means. In the general sense it means studying the less direct effects of technological innovation. But within that rather broad description there is a great variety of interpretations and approaches, even amongst my colleagues. Michael Gibbons, for instance, spent a year's leave with the Science Council of Canada doing a technology assessment of off-shore hydrocarbons off the eastern coast of Canada. He is now back in Manchester and is beginning to do something similar for the North Sea. Harry Rothman, another one of my colleagues, takes a very different approach to technology assessment, a more consumer oriented approach. A year ago he published a book on the bread industry, pointing out certain deficiencies in our knowledge of the effects of some of the additives in bread. His group drew attention also to deficiencies in labelling. One very simple thing they did was to go round Manchester shops and ask for brown bread and then have it analysed. They got all sorts of things under the name brown bread. Some of it was wholemeal, some of it was wheat germ-enriched and some of it was just white bread with brown colour added. Consumers deserve better information than that.

Harry Rothman has now moved on from bread to the other end, so to speak — to sewage disposal. Yes, there are endless jokes about that, and a fair amount of crude language, but Harry and his colleagues are in deadly earnest about it. They see the recovery and use of faeces as a very serious and important development which has to come sooner or later. They have been collaborating with Simon-Carves, the Manchester-based engineering firm; patents have been taken out, a company has been formed and talks are under way with water authorities. One of the main technical difficulties is the removal of metals from the sewage. Faeces are a good source of first class protein, all of which is going down the drain at the moment. One way of using that protein would be by interposing some other animal between it and its re-use in man; for instance, one possible way is to mix the sewage with chalk and straw and use it as poultry food. There are emotional objections to this sort of thing, so to show how safe it would be some people have actually eaten biscuits baked from faeces and shown that there are no obvious ill effects.

Let us move on to something less earthy. I suppose the intellectual centre of gravity of my department now is the sociology of scientific development. For instance, how do new specialities emerge in science, or indeed in any kind of knowledge? How does knowledge become institutionalised? In what ways do changing social pressures reflect themselves in the institutionalisation of new bodies of
knowledge? One current study concerns textile science. How did textile science become incorporated into the academic system? What were its relationships to the industry at different stages in history? A similar study on very different subject matter concerns the emergence of parapsychology, in which there is a good deal of interest now. Scientific controversies are good material for sociological analysis and there has just been published a book by one of my colleagues on the 'Race and IQ' controversy considered from a sociological point of view. And there are various other projects going on. We have got people from India and Canada studying Indian and Canadian science policy respectively. We have of course got people interested in alternative technology or appropriate technology. One of the most intriguing of these is somebody from Jamaica who is studying the transfer of folk medicinal practices from Jamaica to the United Kingdom. In our blurb this is called ‘cultural transfer’, which is as good a name as any I know for studying the use of cannabis.

We have several leaflets describing the department’s activities and a list of recent publications. These will give you further details of our activities.

Discussion

Clark-Walker: Why was the word ‘Liberal’ used in the name of your Department?

Jevons: That is a good question. Flowers, who helped to start it all, offered a bottle of whisky to anyone who could think of a better name. As far as I know the offer is still open. ‘Liberal Studies in Science’ is really very nondescript and about every two years we sit down and try to think of a better name. The name ‘Science Studies’ is used by Edinburgh but that doesn’t tell you anything either. I have now come to realise that there is a genuine value in having a name that doesn’t mean anything because it enables one to change as time goes on. Our interests and emphases have changed in many ways since 1966.

West: In the same way the name of the department ‘Experimental Pathology’, in which I am is also a good name. ‘Experimental’ because the work is laboratory based, ‘Pathology’ because we are involved with the study of disease processes but before you study that
you have to know a lot about the healthy state in humans and in animal models. So the name allows quite a wide latitude.

Evans: You are probably sick of people taking issue with you on your salad analogy which I thought was a beauty. I wonder if I might ask you whether disciplines are the only things that can be served up as solid bits of salad. Can other things such as particular questions and particular issues be served as valuable undergraduate courses or parts thereof?

Jevons: I agree with you that that is the main alternative and the obvious example to take is environmental-type issues which over the past few years have been very popular. When you get down to it, though, it seems to me that if you want to seriously discuss environmental issues you have to use the existing disciplines.

Evans: The point I am making is that there are various ways that information frameworks and so forth are put together in courses and you might get two extremes. In an environmental course, for example, you might get a stretch of lectures from the chemist, a stretch from the physicist, a stretch from the economist and a stretch from the sociologist. Another alternative which we have been adopting in the Human Sciences Programme here in the ANU is to take the risk and let one person put on the whole course and thereby try to integrate the whole thing lecture by lecture — not necessarily everything in every lecture, but not keeping the disciplines separated out. There are still good solid chunks in something like this.

Jevons: In the first approach, you are really leaving it to the student to do the integrating aren't you?

Evans: Yes.

Jevons: That always seems to me to be pretty tough on the student because you are asking him to do what none of the staff has been able to do. You are saying to him, 'I am sorry, we haven't got the time, but we expect you to do the integrating by the time you come to your final exam'. I am not sure how fair that is on an undergraduate. It seems to me that you have to perform some integration at the postgraduate level before you can expect the undergraduates to do it.
Evans: Yes, my feeling is that this is a very hard task and in the first type of course it is completely neglected most often. In the second kind of course it can at least be taken up and shared by both the teacher and the students.

Jevons: Yes, but the parity between teacher and student raises a very interesting point. Some of the people who argue for new types of knowledge systems suggest that reactionaries like me who argue for the retention of something like some of the old disciplines are merely trying to preserve the power structure. The physicists and chemists are the ones who know the physics and chemistry and because of that they are the ones who retain power in the educational system. The protagonists of new knowledge systems suggest that the main barrier to innovation in the curriculum is reluctance to let the power be shared with the students. But I think that argument is quite upside down. A student in an environmental course who wants to use a chemical technique to study an environmental problem is even more at the mercy of the specialised chemist because he knows even less chemistry than a chemistry undergraduate.

Wallington: Could I say that, in the study of environmental problems, people from individual disciplines are asked to study particular aspects of the problem and when the data from all the disciplines are put together, the result is not altogether satisfactory. What you need is to have someone to weigh up all the individual contributions bearing in mind the overall nature of the problem. It seems to me that there aren’t very many people capable of such a task. I would like to see some change in the educational system that would make the system more capable of producing such people.

Jevons: It is difficult, isn’t it, because there are not many people around at the teacher level who have ever achieved it. There is another venture at Manchester which is almost entirely separate from us although we do have some contacts with it. This is the Pollution Research Unit which runs the M.Sc. course in Pollution and Environmental Control. They have set themselves the rather tough job of taking either Science or Social Science graduates. Entrants to the course could be engineers, botanists, economists, etc. The first two or three weeks of the course are used as ‘levellers’ to give them those parts of the background they do not have. I think the course has been reasonably successful. There is only a small central staff including a director and two others; the remaining
teachers for the course are drawn from various parts of the university.

Fildes: What proportion of your Ph.D. students have been graduates in the natural sciences and what proportion graduates from the social sciences?

Jevons: With one or possibly two exceptions, they have all done some science at the undergraduate level. A few of them come from our own undergraduate course but not many. Most come via our M.Sc. course — that is, they have taken a first degree in science or technology or a degree which includes some science or technology. They take our M.Sc. programme as a preparation for their Ph.D. research in the department and it really doesn't make much difference to the course length as the M.Sc. course work takes only six months. But that six months is pretty concentrated and it makes a lot of difference to their ability to see things in a new light. In our very first years, we did not have the M.Sc. programme and we had to admit people straight to the Ph.D. That was not nearly as satisfactory.

Slatyer: One thing which I thought you might have drawn out is that during the years that these courses have been offered some central strategy might have emerged on how the projects themselves might have been handled. I think you said that there were 33 projects going at any one time and it would seem to be impossible to retain a central core of interest in seminars, for example, unless there was some central strategy, which a considerable number of people were employing. For instance, when discussing scarce resources you used as an example the relatively insignificant effect of the cost of mercury on chlorine production. In this study a sensitivity analysis type of strategy may have been used and perhaps one of the central functions of the project would be to highlight the relative importance or lack of importance in processes of factors that the general public might find counter-intuitive to what they think should happen.

Jevons: I agree with your diagnosis of the difficulty and it is certainly true that I myself can no longer keep a grip on the wide diversity of projects going on. 33 is the right number — that is the number of projects listed in our current duplicated set of sheets entitled 'Research Activities — October 1974'. There is some duplication and exaggeration for public relation purposes but there are still 15-20
genuinely different projects as diverse as mercury scarcity, the institutionalisation of parapsychology and sewage disposal. It is hard to get common ground and I don’t think that one could say that there is a common strategy.

Harris: Could I just comment on that example of mercury scarcity, since you did quote it as an example of where the price mechanism did not seem to be working. I would question whether that is necessarily so. An important issue is what substitutes for mercury are available in the market and there are a lot available. It could well be, therefore, that the price mechanism is working very effectively. The real problem with the mining industry as far as mercury is concerned is that it is extremely expensive to prove out reserves. The consensus among the technologists in the industry appears to be that there is not a shortage of mercury per se but that there is a very real cost involved in proving more than it is absolutely necessary to prove. So in fact the price mechanism may be acting very rationally reflecting the almost surplus situation in world markets in mercury at the present time. I think this does relate to what Ralph (Slatyer) was saying. The kind of problem mentioned is an example of derived demand, where a basic input is only a very small part of the total cost. This is a very common economic phenomenon — it is one of the things that economics students do study. It is very relevant for example in the demand for wool in Australia — it is one of the reasons we get a very odd kind of reaction, a counter-intuitive reaction as he put it, in wool price formation and in wool demand.

Jevons: Have I got you right? Are you saying that what the price mechanism shows is something which is known to be true on other grounds — that there is no physical shortage of mercury?

Harris: Yes, the results your student found may reflect that the price mechanism is in fact working extremely rationally.

Jevons: There is still a question about the time scale of responses because these things are in the hands of firms which have no particular interest in looking more than 5 or 10 years ahead. If you did run out of mercury, you can make chlorine without mercury.

Harris: The mercury processors would have an interest in making sure that they had a sufficient availability of proven reserves of mercury to cover their investment programme. In the event that the
substitution of mercury became necessary, the time taken to develop new technology may become very critical.

**Jevons:** This is one of the areas where we really do need more information and it is an area where science policy people should be more active. What are the alternatives to mercury in all those areas where mercury is now used and how soon could they be brought into operation? That is an important technology policy problem.

The main concern of businessmen and industrialists is the price fluctuations in minerals such as mercury and copper. What they really require is a stable price; the actual level of that price is of relatively little consequence but the price fluctuations are difficult to cope with.

Leaflets describing the work of the Department of Liberal Studies in Science:

1. The Honours School of Liberal Studies in Science.
2. Undergraduate Theses.
3. M.Sc./Diploma Programme — Structure and Organisation of Science and Technology.
4. Research Activities.

Some recent publications:

10. R. Johnston and M. Gibbons, ‘Characteristics of information usage in


'Science and Literature' is not sciency policy in any narrow sense but it is relevant if you consider the integration of science into culture, the relationship between science and other cultural activities, as being part of science policy in a wider sense.

Few serious authors have dealt with science and scientists — so few as to leave me rather puzzled. It is possible, I suppose, that science is by its nature unsuitable as a subject for literature. Many people clearly think that. But I have a feeling that it shouldn't be unsuitable, because it is not in fact a value-free, impersonal activity, it is intensely personal, intensely involved with the society in which it is going on, as is becoming increasingly recognised. Possibly an explanation for my puzzle is that this recognition has come only recently. We are only just emerging from the period when 'logical positivism' had an icy grip on the philosophy of science and encouraged the view that science is impersonal and asocial. This possible explanation encourages me to hope that we can now look forward to a richer period for science in literature.

In the recent literature, there are of course the novels of C. P. Snow, in some of which scientists are quite prominent. All of Snow's Cambridge novels include some scientists. In *The Masters*, for instance, which deals with the election of a new master at a fictitious college, one of the key characters is the relatively unsuccessful scientist who eventually decides to vote for the distinguished scientist because he thinks that will help him to get elected to the Royal Society. *The Affair*, which is set in the same college, deals with scientific fraud. Fraud is a very rare occurrence in science, as you know; to explain this, it is not necessary to suppose that scientists are intrinsically more honest than non-scientists, but only to remember that it is difficult to get away with fraud in science because sooner or later somebody can usually do the same experiment.

There are of course widely varying estimates of Snow's qualities as a novelist. Many people don't think at all highly of him. I have a
tentative theory of my own that the more science there is in one of his novels, the more wooden the construction is. It is years since I read *The New Men*, which deals largely with the atomic scientists of the immediate post-war period, but I do retain a vivid impression of it as even more wooden than the other Snow novels. On the other hand, *The Conscience of the Rich*, which has virtually no science in it, but deals with Anglo-Jewish families in London, seems to me to flow rather smoothly, and is, I think, one of his better novels.

Arthur Koestler is another author who is intensely interested in science. He writes about scientists in the historical fashion. Some would prefer to say he writes in a pseudo-historical fashion, but I have a high regard for *The Sleepwalkers*, Koestler's treatment of the revolution in astronomy, centering on Copernicus, Tycho, Kepler and Galileo. In *The Act of Creation*, Koestler attempts to analyse scientific creativity. Neither of these two books, however, is really a novel, despite what some critical scholars say. In Koestler's novels, scientists do not figure largely. The best of them is *Darkness at Noon* (about which, incidentally, there is a sick local joke in Manchester — its subtitle, it is said, should be *August in Manchester*). *Darkness at Noon* is an excellent book, but it has no natural science in it; it is a straight political book.

Doubtless there are other and perhaps better examples of science and scientists in literature. Rather than attempt a comprehensive survey, I would like now to discuss one particular work in which I have been interested: Brecht's play *The Life of Galileo*. It has become something of a classic and seems to me to repay analysis.

**Brecht's life**

I will start by telling you a bit about Brecht, because what he says is clearly not unrelated to the context in which he says it. Brecht is considered by many of the cognoscenti to have been a major force in the modern theatre. He was born in Bavaria in 1898, of a good, solid middle class family. There is an amusing story about him at school. He and his friend came bottom of their class in French and Latin respectively. His friend rubbed out some of the corrected passages and asked to have his work marked again. He was soon found out. Brecht was more subtle. He marked some extra passages wrong in red and then asked the teacher why they had been marked wrong. The teacher had to admit there was nothing wrong with them and raised his mark.

He began to study medicine and science at the University of
Munich but his studies were interrupted by the war. The chaos, unemployment, hunger and misery of the 1920s probably helped to turn him Communist, though he was not explicitly a Communist until about 1930. In 1933, when Hitler came to power, he had to flee for political reasons and he settled in Denmark during the period 1933 to 1939. In Denmark he had contact with the physicists in Niels Bohr’s laboratory, which made Copenhagen into one of the world centres of nuclear physics. Brecht acknowledges the help that these physicists gave him in reconstructing the Ptolemaic system of astronomy, the ancient system which Copernicus and Galileo overthrew. The first version of *The Life of Galileo* was in fact written in Denmark in 1938 to ’39.

In 1940, when Germany invaded Denmark, he went to Finland and then to Russia; he didn’t stay in Russia but went right across it to the United States. The English version of *Galileo*, which was an appreciably modified version, not merely a translation, was made in collaboration with his friend Charles Laughton, the famous Hollywood film actor, in California. Laughton himself took the title role in the 1947 production. In that year Brecht returned to Europe and founded the Berliner Ensemble in East Berlin. The reputation he built up is based as much on the very high standards of production and acting of the Berliner Ensemble as on his written plays. He was rehearsing a production of *Galileo* when he died in 1956.

There are some features of his life which have been used to cast doubt on the sincerity of his Communist convictions. In 1933, when he had to get out of Germany, he didn’t go to Russia but to Denmark. In 1940 he actually was in Russia but he chose not to stay there but to go on to the United States. In 1947 he did choose East Berlin to go to but he acquired an Austrian passport and a bank account in Switzerland, just like a wicked Western Capitalist. Facts like these about his biography raise an issue relevant to the interpretation of *Galileo*. Are there absolute standards in ethics or is it better to adapt to circumstances as you find them? In Brecht’s case, do you stick to your Communist principles or do you bend them slightly for your convenience? In Galileo’s case, was he justified in recanting in public in 1633 so that he could get on with his scientific research? In brief, is martyrdom or adaptation the better course? The Galileo of the play, like Brecht in real life, preferred adaptation.
Theory of the theatre

Brecht had a theory of the theatre, a peculiar dramatic theory, the catch word in which is 'Verfremdungseffekt'. 'Fremd' means 'strange', and although the usual translation is 'alienation effect', a more accurate rendering would perhaps be 'estrangement effect'. According to the theory, the purpose of theatre should be to present things on the stage so as to be recognisable but to appear in some way strange. That is supposed to arouse in the spectator a desire to alter it. In that way the play can become a vehicle for Communist propaganda, because it is assumed that if the spectator wants to change society he will want to change it in the direction of Communism.

This view of theatre is very different from the conventional. Conventional theatre does not aim to detach the spectator from the action on the stage but to involve him emotionally in it. A stage atmosphere is created which aims to draw the actors and spectators together. Brecht, by contrast, wanted the audience to dissociate itself. The old form he called the dramatic form, or Aristotelian form. Here the function of the theatre is to provide emotional catharsis; it purges spectators of their feelings and uses up their emotional energy, rendering them harmless members of society with no further desire for action. After the emotional experience of the play, their revolutionary potential is sapped. Brecht's new theatre was to be epic rather than dramatic and to arouse in spectators a desire to change society. In 1931 Brecht drew up a scheme contrasting the two forms; although it is obviously over-simplified, I give an abbreviated version in Table 1. In the old form the spectator

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<th>Dramatic form of theatre</th>
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<td>involves the spectator in the stage action</td>
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<td>he is drawn into something</td>
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<td>suggestion feeling man unalterable</td>
<td>argument reason man alterable and altering</td>
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is supposed to be involved, in the new form he is supposed to be a detached observer. The old form consumes his capacity to act, the new form awakens it. The old form allows the spectator to have feelings, the new form demands decisions from him. In the old form the spectator is drawn into something, in the new form he is confronted with something. The old form depends on suggestion and feeling, the new form on argument and reason. Lastly, in the old form man is presented as something unalterable, human nature is given and permanent, whereas in the new form man is alterable and in fact seen to be altering; in other words, he adapts rather than making a martyr of himself.

Brecht was dissatisfied with the tragic outcome of so many classical plays. To accept tragedy seemed to him to be fatalistic. He wanted to take a more optimistic view and show that man is capable of avoiding tragedy. And that, on one plane at least, is what Galileo does by recanting, by being adaptable rather than becoming a martyr.

Obviously the scheme cannot be taken in an absolute, one hundred per cent literal sense and Brecht himself later softened his attitude. If the spectator is not involved at all with the stage action, if emotion is kept out of it altogether, if he doesn’t identify with the characters on the stage in any way, then there is no point in going to the theatre. The involvement is, however, supposed to be an intellectual rather than an emotional one. Brecht wanted to regard the drama as playing a didactic role, its function being to teach, not to entertain.

The expression ‘Verfremdungseffekt’, ‘alienation effect’ or ‘estrangement effect’, seems to have been first used by Brecht in a note on Chinese acting, which was occasioned by seeing a Chinese company in Moscow during a visit in 1935. Chinese acting is highly stylised. The actor, Brecht noted, never acts as if there were a fourth wall besides the three surrounding him. He expresses his awareness of being watched, he deliberately chooses his positions to show himself off to the audience. He is not trying to play naturalistically. ‘The audience can no longer have the illusion of being the unseen spectator at an event which is really taking place.’ ‘The artist’s object is to appear strange and even surprising to the audience. He achieves this by looking strangely at himself and his work.’

In his own production, Brecht introduced deliberate artificialities. He might have the titles of the scenes projected on the backcloth of the stage. In his early productions he sometimes had the gramophone which was producing the sound effects visible on
the stage, but people just laughed at that, so he gave it up. All these deliberate artificialities were supposed to stop the audience from falling under the 'spell of illusion'. Similarly, Brecht wanted the actor to keep at a distance from his part. Some of the famous acting methods like the 'Stanislavski Method' are, as I understand them, really means to help the actor to step into the part, to completely identify himself with his part. For a classical actor it would be high praise if you said of him that he didn't merely act Hamlet, he *was* Hamlet. Brecht wanted the opposite. He made his actors practise by translating speeches into the third person, beginning 'He said' (I can imagine some comic scenes at rehearsals: 'He said, was it better to be, or was it better not to be . . .'). In a striking image, referring to Charles Laughton's portrayal of Galileo, Brecht said that a good actor acts as though he had just put his cigarette down for a moment to show us a scene from the play.

These are tricks of production and acting, not given in the text, but the same points are made in the notes that Brecht wrote for the play. Thus in Note 1 to *The Life of Galileo* he writes: 'The stage decor must not be such that the public believes itself to be in a room in mediaeval Italy or in the Vatican. The public must always remain clearly aware that it is in a theatre'. And in Note 7 he says: 'The characterisation of Galileo should not aim at establishing the sympathetic identification and participation of the audience with him; rather, the audience should be helped to achieve a more considering, critical and appraising attitude'.

**Content of the play**

Since Brecht saw the function of the theatre as an intellectual and didactic one, it seems reasonable to me to look at the intellectual content of the play to see what he says there about science and about the relations between science and society. What I would like to do is first to take four small points and then to discuss four of the main issues raised in the play.

1. First, there is the role of external factors. Brecht emphasises the importance of factors external to science in helping to direct and guide the direction and pace of scientific advance. In the 16th and 17th centuries, the impetus for research in astronomy came partly from the need for calendar reform but even more perhaps from the needs of navigation. At the very beginning of the play, Galileo is talking to his young pupil Andrea, who at this stage is only ten years old. He says, 'I like to think it began with ships. Ever since men
could remember they crept only along the coasts; then suddenly they left the coasts and sped straight out across the seas.'

The argument about the relative importance of internal and external factors is to me one of the most interesting features of the history of science. Classical history of science was intent on tracing the history of ideas, seeking the origin of new ideas largely in earlier ideas. But 'external' historians (Marxists have made notable contributions here) tend to look rather for social factors, factors outside science itself, directing science. To some extent at least they are right, I feel sure. Remember that the great age of discoveries had come at the end of the 15th century — Columbus' first voyage was from 1492 to 1493 — and in the 16th and 17th centuries there was great activity in navigation. Navigational problems such as how to find positions at sea were immensely important. Perhaps the problems of navigation then could be compared with the problems of space now. Everybody recognises that the space race has had a very important stimulating and directing effect on the advancement of technology. Similarly for navigation in the 16th and 17th centuries.

2. Second, Brecht was clearly intrigued by the questions of the justifications for science and the patronage of science. His comments are largely fair ones. Note, for instance, the reactions of the senators of Venice when the Curator presents the newly invented telescope to them. When he mentions prestige, the prestige that Venice will derive, he gets 'polite applause'; the commercial possibilities of the telescope draw 'stronger applause'; and the military value of the telescope elicits 'very loud applause'.

In 1610 Galileo moved back from Venice, where he had been working for eighteen years, to Florence which was home to him. He thought very hard about that decision, and we have letters in which he discusses whether he should go or not. In the play, Galileo says, 'Your protection of freedom of thought is quite a profitable business. By pointing out that elsewhere the Inquisition rules and burns, you get good teachers cheap.' But, he goes on, 'what use is freedom of research without free time in which to research?' The Curator's reply is that at least he is free to work in Venice, and if he wants more money he could invent something useful. One of his friends warns him of the dangers but Galileo goes to Florence nevertheless because free time in which to work is what he wants most. Brecht is making the point — a fair one, surely — that conditions of work are terribly important to scientists.

3. Brecht also comments in more than one way on the
popularisation of science. In scene 1, as I mentioned, Galileo lectures a ten year old boy. He says to Andrea, 'I particularly want you to understand it' (that is, the main astronomical issue). By picking a ten year old boy as Galileo’s audience Brecht was trying to emphasise, I think, the presentation of scientific issues to non-experts, to the lay public. Galileo wrote some of his propagandist works in Italian rather than in Latin, and in the play the Inquisitor complains about this to the Pope. ‘This wicked man knows what he is doing when he writes his astronomical works, not in Latin, but in the language of the fishwives and wool merchants.’ The Pope agrees: ‘That shows very bad taste; I will mention it to him.’

Popularisation is an issue that must be dear to every good Marxist’s heart. Every good Marxist wants the whole population to understand, so that science is not an elitist activity but an activity for the people as a whole. I am told that one of Galileo’s recent biographers, Geymonat, was influenced by Brecht’s interpretation of Galileo’s actions regarding popularisation.

4. One of Brecht’s best and most perceptive points concerns the relation between science and authority. He says that the Church is ‘portrayed as a secular authority, its ideology is, fundamentally, interchangeable with many others.’ Brecht clearly had no reverence for the Catholic Church. Nevertheless, he said, ‘this play must lose a great part of its effect if its performance is directed chiefly against the Catholic Church.’ ‘The Church functions . . . simply as Authority.’ ‘It would be highly dangerous, particularly nowadays, to treat a matter like Galileo’s fight for freedom of research as a religious one; for thereby attention would be most unhappily deflected from present-day reactionary authorities of a totally unecclasiastical kind.’ Brecht was clearly thinking of political interference by the Nazi regime in Germany with freedom of research.

Freedom of research and the relativity of ethics

That brings me to what I see as the main issues of the play. Two of the main issues, I think, are the freedom of research and the relativity of ethics. There is tension between these two over Galileo’s recantation. One has to distinguish here between the historical situation and the situation in the play, because Brecht was not writing history but taking liberties with history. The historical situation regarding Galileo’s recantation is extremely complex. But in the play the situation is simplified: Galileo just recanted his
teaching of the earth's motion on being shown the instruments of torture. Was he justified in doing so? Was it sheer cowardice, or was he justified in recanting so that he could get on surreptitiously with writing *The Discorsi*, the Discourse on Two New Sciences, his great work on physics, as distinct from astronomy?

The basic issue is the one I have already referred to in connection with Brecht's own life: is adaptation preferable to martyrdom, or should one have the courage of one's convictions and be a martyr? Is it better to stick to one's standards or to adapt chameleon-like to what the environment happens to be? On the face of it, Galileo in the play preferred adaptation, as did Brecht in real life. One can think of Galileo as having outwitted the authorities by paying lip-service to them while quietly getting on with his really important work on physics. At the end of the play, in scene 14, Andrea, now grown up into a young man, visits Galileo again and becomes convinced that this is what happened. Seen in this new light, Galileo by a clever trick, has managed to emerge the victor. Full of admiration, Andrea tells him: 'You concealed the truth. From the enemy. Even in the field of ethics you were a thousand years ahead of us.' Galileo interprets this as 'New science, new ethics.' Ethics can change; with a new science can come a new ethics.

Galileo had been ruthless in what he had been prepared to do in order to be able to get on with his research. He had not obeyed other ethical standards in his fight for freedom of research. Thus he had perpetrated something of a swindle on the Venetian authorities about the invention of the telescope by passing it off as his own invention (in the play, that is, although not in history). He ruined a good match for his daughter Virginia rather than let it interfere with his research. In short, Galileo was grossly selfish for the sake of his research. All other standards of ethics had to give way to his demand for freedom of research. As I said earlier, Brecht felt that rigid adherence to absolute standards makes for tragedy. He wanted to shown man as capable of avoiding tragedy by his wits and his adaptability. Man was to be presented as 'alterable and altering.' 'Misfortune', Galileo tells Andrea, 'comes from insufficient foresight. Taking obstacles into account, the shortest line between two points may be a crooked one.' Note the intriguing double meaning of the word crooked.

The pleasure of research

In the later versions of the play, from 1947 onwards, these issues are
overlaid with others which make the situation more complex and more subtle. Thus in the later versions Brecht emphasises that science is enjoyable. The urge for research is a pleasurable urge; doing research can be seen as a form of self-indulgence or self-gratification. I suspect that Charles Laughton helped to introduce or at least reinforce this element in the play. I can imagine Laughton as Galileo eating his goose in scene 14 with the same kind of relish that he brought to his famous portrayal of Henry VIII, wolfing chicken legs and tossing them half eaten over his shoulder. Scene 14 contains the big speech of explanation, the intellectual climax of the play, immediately after which Galileo says, 'I still enjoy my food.' Similarly in other parts of the play. Before he moves to Florence he says to his friend, 'I want the fleshpots ... I despise people whose brains are not capable of filling their bellies.'

Brecht and Laughton were at pains to link this sensual pleasure with the intellectual pleasure of research. In his notes Brecht says, 'Just that very mixture of the spiritual and the physical interested Laughton. Galileo's physical pleasure, when the boy rubbed his back, was transmuted into intellectual creativeness.' In the first scene Galileo is presented naked to the waist having his back rubbed by ten year old Andrea. The implication must be that some people get kicks out of having their backs rubbed by ten year old boys. 'Thus Laughton emphasised that Galileo is once more enjoying his wine when, in scene 9, he hears that the reactionary Pope lies dying. His relaxed way of walking up and down, and the play of his hands in his breeches pockets when planning new researches, verged on the shocking.' The same point is made a number of times in the text of the play. Galileo says, 'thinking is one of the greatest pleasures of the human race.' He describes the theory of the tides as 'an apple from the tree of knowledge.' The Pope says of Galileo: 'He knows more pleasures than any other man I have met. He even thinks from sensuality. To an old wine or a new idea he cannot say no.'

Social effects of science

Another complication in the later versions of the play concerns the social effects of science. Perhaps I should call them the anti-social effects, because the revision of the play in 1947 was done under the shadow of the first atom bombs at Hiroshima and Nagasaki. Despite the great relief at the end of the Second World War, there was a wave horror at what science had unleashed on mankind. The Japanese War had been very costly and nasty; the impending
invasion of Japan, which was the alternative way of ending the war, would probably have cost more lives than did the atom bombs. Nevertheless, public reaction to the bombs in Western countries was as much one of revulsion as of gratitude. Public attitudes to science have always been mixed. Sometimes science is seen more as the bringer of good things, at other times it is seen more as a source of evil. The balance between the two views changes over time. In the late 1940s there was a strong anti-science feeling because of the atom bombs. It was the destructive rather than the productive potentiality which predominated in the public image of science. People were not saying that science can save us but asking who could save us from science.

If you look at the Galileo of the play in this light, you will find that he is unscrupulous, not only about the means that he adopts so as to be able to get on with his work, but also about where that work may lead. ‘I would suggest’, he says in scene 4, ‘that as scientists it is not for us to ask where the truth may lead us.’ It is left to the philosopher to object, expositing furiously, ‘Signor Galilei, the truth may lead us to absolutely anything.’

In scene 8, an anonymous little monk comes to tell Galileo that he has seen the wisdom of the Church’s decree because he can now see ‘the danger to mankind that lurks in too much uncontrolled research.’ The danger he sees is to the peace of mind of the people, the poor, the peasants; doubt, he says, will take the meaning out of their misery and will make that misery unbearable. It is only faith that makes life bearable. Galileo agrees only in so far as it is people, not planets, who are the real issue. ‘You are right’, says Galileo, ‘it’s nothing to do with the planets, it’s to do with the peasants.’ But the link that Galileo sees is a different one. It is more with the material welfare of the people than their spiritual welfare that Galileo is concerned. He says, ‘We cannot invent machinery for pumping up the water from the river if we may not study the greatest machinery that lies before our eyes, the machinery of the stars.’

The key analysis is given in Galileo’s long speech in scene 14. ‘Could we deny ourselves to the crowd and still remain scientists?’ he asks. His crucial betrayal, it seems, was not just betrayal of the new discoveries; it was not even the betrayal of reason and the new scientific method and the criteria of truth which the new scientific method represents. They would have won anyway; ‘there is no scientific work that only one man can write.’ The crucial betrayal was the failure to forge the links that would keep science and society in step. Galileo maintains that ‘the only purpose of science is to ease
the hardship of human existence. If scientists, intimidated by self-seeking people in power, are content to amass knowledge for the sake of knowledge, then science can become crippled, and your new machines will represent nothing but new means of oppression.

I think this is partly what Brecht had in mind when he spoke in his notes of Galileo’s crime as ‘the original sin of modern natural science.’ What he seems to have had in mind is that the technical proficiency of science is won at the expense of its divorce from social needs. ‘The atom bomb is, both as a technical and as a social phenomenon, the classical end-product of his [Galileo’s] contribution to science and his failure to society.’

That, roughly, is what I made of the play when I tried to analyse it. However, several people pointed out to me after I had published a short article on it that I missed an important aspect — perhaps the most important aspect. When Brecht talks of science and society he didn’t refer only to applications of natural science which strongly affect society, such as the atom bomb. As a Marxist he also meant that we should develop a science of society, a scientific sociology. That is the point which M. Cohen makes in his rejoinder to my paper. The same point is made more briefly in a letter that I have from Martin Esslin, head of radio drama for the BBC in London. Esslin had written a book on Brecht and I sent him an off-print of my paper. This is the letter he sent me in reply on 4 February 1969: ‘Many thanks for sending me the off-print of your article on Brecht’s *Galileo*, which I have read with great interest. I agree with all you say. As regards your final question on how it was that Brecht imagined the role of the scientist in society, I think the answer is to be found in the fact that rightly or wrongly — in my opinion quite wrongly — he equated Communism and science; that is: a society organised on scientific lines to him was Communism. Thus what he was attacking Galileo for was that by submitting to a non-scientific authority he set a precedent in acknowledging a subordinate role for “science”, thereby depriving the “science of society” founded by Marx of its birthright of instant acceptance.’

Discussion

*Jevons:* Can any of you provide for me some other and better examples of attention paid by literary authors to science?

*Elliott:* I can think of Bertrand Russell, who was very much aware of certain limitations. One of the reasons why he turned to writing
fiction rather late in life, he said, was ‘there are certain things which I cannot say in any way along the lines that I have taken in the past because I cannot justify them rationally, therefore if I want to say them out loud I must put them in fiction form’. I think this is a case of a man whose entire conduct and thought had been along rational, logical lines and he had to try and find a way out because there were certain things he really did believe but which he could not justify. So there is one thing there that we can see: the inability to say certain things in scientific language. To some extent, I think this is the problem we are hinting at when we talk about the responsibility of the scientist. Certain things that scientists do feel, think, and want to say, they can’t really justify on scientific grounds.

Jevons: Can you tell us a bit about what Bertrand Russell wrote?

Elliott: He wrote a number of pieces called Nightmares of Eminent Persons in which he created a situation for a number of major political figures of the time, Stalin and lots of others, and had them experience in nightmare form some of the agonies resulting from their policies. They were usually written as short pieces, three or four pages. There are also one or two longer short stories and quite a number of shorter short stories. Most of them are concerned with putting ideas in fictional form and his own nightmares if you like, about society. Recurring themes are confrontations between East and West and what governments would do; what happens in a society that is completely taken over by the state machine; what happens to the person who wants to think. Is any kind of thought possible? His theory is that in fiction we can dispense with scientific terms because we are dealing with matters not capable of proof. He turned his theory into a kind of ‘meta-fiction’ and the result is a very interesting justification in fictional terms of the sort of things that he said in his earlier writings and essays. A great deal of the stories are concerned really with issues, ideas, and fears rather than with characters.

Jevons: It is almost inevitable that you reach a nightmare view of science and society if you combine the view that a ‘science of society’ leads to a scientifically run society, with the view that there is only one ‘scientific’ view. I hold to neither of those two views.

Clark-Walker: Edward Teller, when he was asked why he worked on the atom bomb, said that if he didn’t work on it somebody else
would. Similarly with the genetic engineering of today, there are a lot of people who thought that the moratorium was special pleading on the part of people who invented the techniques. In relation to this do you think there is any way in which society can control science?

**Jevons:** I'd like you to tell me the answer to that one. We would have to work on the conscience of every individual scientist. How do you do that, short of a totally totalitarian society?

**Elliott:** You can't do it short of a totally totalitarian society. This is one of the things that as a non-scientist constantly fascinates me — how do you as scientists reconcile the possibility of social damage that your work might possibly bring about with the freedom which you wish for, and which you have a right to assume, for research to proceed? This is the tremendous problem of social responsibility of science. Do active scientists like yourselves ever consider this problem?

**Clark-Walker:** I think we do, but the problem is, as Teller put it, that if he did not work on the bomb somebody else would. It is also not possible for everyone to hold similar views about the necessity for moratoriums on certain potentially hazardous projects so there will always be the person eager to step in and get the personal glory. I think these are the dilemmas that most people find themselves in when confronted with something that is potentially dangerous.

**Questioner:** Was Esslin correct in saying that Brecht was really saying Marxism directly related science with politics?

**Jevons:** Elsewhere in the play, Brecht links the struggle for freedom of research with the political struggle. Science and politics become closely intermingled. Recently, Marxists have been very busy politicising science.

**West:** I was going to make a comment about the atomic bomb. It seems more acceptable when you know it is not going to be used. I have been recalling how I have thought about it over the years. There were certain stages when I knew it was not going to be used, therefore it was quite acceptable; but at other times, when I really thought it was going to be used, I started to feel revulsion towards it.

**Jevons:** I am afraid it can only be a few years before a small atom
bomb is used — they are so easy to make now.

West: It may not be a country that will use it but private individuals.

Jevons: It is likely to be a terrorist group of one kind or another.

References

During the past few years, there have been some quite marked changes in the preferences students show for various subjects. In Britain, as in Australia, there is now less interest in secondary schools for the physical science subjects — maths, physics and chemistry. At the higher education level, it's not only those subjects which suffer, but also engineering and the technologies generally, because they depend on preparation in physical science.

Many people have expressed concern about this swing. I want to examine some possible reasons for it and to ask how serious it really is.

Some people say the reason for the swing is just that today's young generation is soft and spineless. 'They've had it too easy', one hears elderly people saying as the cigar smoke thickens after a good dinner. 'They're not prepared to buckle down to some good honest effort'. And there seems to be some support for this view in what is happening to the study of foreign languages. That is getting less popular too because — so it is said — learning a foreign language is also too much like hard work.

I'm a bit unhappy about this diagnosis. There may be something in it, but I fancy there is more behind the swing than that. For one thing, subjects other than science and languages are not all that easy. Sociology and history and philosophy are not exactly pushovers. Even within natural science, the life sciences, as distinct from the physical sciences, are doing well. Young people want to do biology even if they are not so keen on physics and chemistry.

Furthermore, in my experience there are still plenty of young people who are prepared to work hard at things they believe in. There is still plenty of youthful idealism around to motivate effort — plenty of bellies with fire in them.

I think there is something that goes deeper. There's a feeling of dissatisfaction with the whole mode of life which science and technology seem to represent. Science and technology are seen as the basis of a type of industrial capitalism whose defects are becoming
more obvious. Physical science has become the fallen idol. Scientists and technologists are no longer the folk heroes they were in the 1950s and '60s. They are no longer demigods in white coats, but cogs in a social system which is greedy and messy and ruthless, shortsightedly exploiting the natural environment as well as selfishly exploiting less privileged people within itself.

Science and technology are perceived to be associated — in an admittedly rather loose way — with the blemishes in society. Often it's not a properly developed argument but just a gut feeling that people have — that if that's the kind of life style to which the wonders of modern technology have led, then they want none of it.

So they look for alternatives. For the new societies of which they dream, they want new kinds of knowledge. They read Eastern mystics and practise Zen Buddhism and immerse themselves in Taoist philosophy. They talk about learning to live in harmony with nature instead of exploiting it. And they tend to see technology as the nigger in the woodpile. In their romantic yearning for a simpler and more satisfying life, they envisage a Brave New World without the high technology.

I want to argue that this trend could easily go wrong. It's not that I have no sympathy with the aims — some of them seem to me to be admirable — but I fear the means may be wrongly chosen. The argument regarding technology seems to me to be no more than half right. I cannot accept that technology is intrinsically bad.

What, after all, is technology? Technology is the capability to do things. It's the means we have developed to have some control over our environment. Some sort of technology is a necessary condition of our existence. Without it, we're helpless. The notion that technology is intrinsically bad is, I think, intrinsically absurd.

What is bad is bad technology — and there's plenty of bad technology around. But helplessness can't be a virtue in itself, so we want technology of one kind or another. Consider what would happen if we did abandon our present technology. It's pretty obvious that many parts of the world could not sustain their present populations. Starvation and disease for hundreds of millions of people would be the inevitable result. It seems hardly less immoral than nuclear war.

What alternatives to technology are there? There is much interest now in witchcraft as an alternative knowledge system — an alternative to the scientific style of thought. In September last year I went to the annual meeting of the British Association for the Advancement of Science — the annual event at which scientists set
out to present themselves not just to each other but also to a wider public. Last year’s meeting was at the University of Stirling in Scotland, in a very beautiful setting, and quite a lot of non-scientists and school students came to it. One of the few sessions which attracted more people than the scheduled room would hold was that on witchcraft. Not that most people got what they presumably came for: there were no stories of midnight rites on lonely Scottish moors. What they actually got was abstruse sociological and anthropological theorising. But the drawing power of the session title was symptomatic: witchcraft has become an in-thing. Even the weekly magazine *New Scientist* featured witchcraft on the front cover of the issue which reported the annual meeting.

Consider though: how much of an alternative to science can witchcraft be? It’s possible that some practices from African witchcraft may be effective in curing disease. A conceivable explanation is that a good witch-doctor is skilful at diagnosing social stress and that may in some cases be of real help to the patient. But if the treatment works, it’s not an alternative to scientific medicine — it’s an extension of its range. If the remedy proves its worth, it could and should be incorporated into standard medical practice, as part of the process which should be going on all the time of enlarging our repertoire of knowledge and techniques.

Many of the results of technology have been bad — that’s undeniable. But that’s no argument for throwing it all overboard. What we should do is to improve it and learn to use it more skilfully. It’s not *less* technology we want but *better* technology better used.

One of the troubles about technology is that some of its consequences are unpredictable. DDT, for instance, has been an immensely successful insecticide. It has saved millions from malaria by controlling mosquitoes, and it has saved millions more from starvation by controlling pests that attack crops. Only after it had been in use for years was it shown that it’s dangerous to certain birds. For some reason it makes egg shells too thin and fragile, and the populations of susceptible birds went down. Nobody knew about that in advance, so it’s difficult to argue that it was the fault of any particular social system. We just weren’t clever enough or careful enough. Technology has sometimes been a bit like a bull in a china shop; it needs to become more like a skilled craftsman.

The grand thing about technology is that it’s so versatile: it can be turned to so many different ends. Whatever kind of political or social system we want, or may in the future decide we want, we’ll
need the tools and techniques which science and technology provide. Take, as an extreme example, the kind of society which the zero growth school advocates. Zero economic growth and zero population growth are said by them to be necessary, otherwise the world will grind to a halt for lack of non-renewable resources, or be engulfed by pollution, or face starvation, or meet some other catastrophe. For devotees of the zero growth sect, the high priests are Meadows and his colleagues, formerly at the Massachusetts Institute of Technology, and the bible is their best-selling book *The Limits to Growth*. The computer printouts in that book prophesied doom by next century if mankind doesn’t soon mend its exponentially growing ways.

Now, if you look at the conditions which are necessary for a stable world, according to the *The Limits to Growth* study, you find that lots of technology is needed: it’s needed for resource recycling, for pollution control, for soil restoration, and so on. Meadows’ argument was not an anti-technology argument, as some careless commentators have wrongly suggested; his argument was that technology by itself is not enough.

What kind of technology would be needed in a stabilised world not heading for catastrophe? It would undoubtedly differ in many details from the technology we have around us now, but unavoidably it would rely on much the same principles and components and skills as those in use at present. We would still use wheels, and transistors, and many of the same chemical compounds. The same underlying technology, in other words, can serve both the god of growth and the fad for zero growth.

For reasons like these, I hope most sincerely that not too many able young men and women will turn their backs on technology. It seems foolhardy to talk about replacing the technological tools and techniques we have created when we have nothing of comparable power to put in their place. The wise course, surely is to extend technology so as to widen the range of our capabilities, and to redeploy it to meet new kinds of social objectives, and to refine it to make it a more sensitive and subtle instrument to help us achieve whatever purposes we may collectively decide on.

With good technology, we can argue about what kind of social system we want to build. Without it, we don’t have much choice.
References

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Role of the social sciences in science policy

Selby-Smith: What is the appropriate role of the social sciences, as you see it, in the formulation of science policy?

Jevons: I suppose you are worrying about the way in which natural scientists often tend to assume that it is all a matter for them. I share your concern over that because many of the problems are in fact social science problems. The biggest science policy unit in Britain — the Science Policy Research Unit at Sussex University — is headed by someone whose background is in economics (Professor Christopher Freeman). In Manchester our staff is entirely 'switched scientists' using 'scientist' in the older and narrower sense of the word which was 'natural scientist'. We have adopted that policy because we have access to social scientists. We are sitting in the middle of a large university. If we want inputs from economics or sociology there are plenty of economists and sociologists around. If we were setting up an independent research unit, I am sure we should have social scientists in it as well as people with a natural science background. The sociology of science — a rather dubious though interesting emerging field — is populated partly by genuine sociologists, with all the advantages and disadvantages that genuine sociology brings. Among the disadvantages is the vocabulary which tends to be rather turgid. Problems of resource allocation are susceptible, in theory at least, to examination by economic techniques. Some of the big problems, however, don't fall within the area of any well established social science. I would like to know what you think about this question.

Selby-Smith: It seems to me that science policy studies should encompass social scientists as well as natural scientists. On the other hand they seem to approach problems in a different way — they
have a different background and use different techniques. To what extent do you find this productive and to what extent do you find this counter-productive? Maybe there are certain parts of science policy research where a natural science perspective may have proved more productive, or in your opinion would be more productive, and maybe other areas where a social science perspective might on balance be more useful, and yet other areas where a mixture of the two is required.

Jevons: People are more confident in tackling certain areas if they know the technical content, I am sure. One classic story from the debate on high energy physics in the United States concerns the Senator who expressed strong views (I forget on which side) without, as became very obvious, knowing the difference between a particle accelerator and a nuclear reactor. That sort of thing tends to make the debate lose credibility. I suppose it is true to say that natural scientists do not, by virtue of their natural science expertise, have within themselves the tools to tackle the problems of science policy because such tools are not natural science tools. It would be good to hear from some of our distinguished scientists who have found themselves practising science policy — Professor Fenner or Professor Robertson.

Robertson: I am not sure that I interpret the question in the way that Freddie [Jevons] means me to. However, the sort of thing that one comes up against is that there are a good many laymen that have no idea what you are talking about and that, of course, is a very great difficulty because you are now not talking to social scientists but to the administrators. I have had the feeling, at times, that if some of the people who are interested in the techniques of the social sciences are able to aid us to bridge the gap that exists between the intelligent layman who is in an administrative position and the scientist presenting the case, we would be helped enormously. I think the difficulty is decreasing because more of the non-scientific public has some knowledge of science. However, my experience has been that what social scientists of a sociological type study tends to be rather remote from the functional problems of dealing with science in our community. That is only one facet of the field. There is a much more complicated area in which the aim of us scientists is to tackle most of what we do with what you may call rational thought. There are large sections of the community who just don’t know what that means, let alone practise it. This is not merely a question of intelligence level.
There are some people who are not bright enough to know what it means but there are others who are so much in a particular psychological ‘gestalt’ in which they have been brought up that they can’t look outside that framework. The spread of useful science in the community is going to override that. I have made a speech instead of answering a question.

**Questioner:** The question originally asked about the role of the social sciences in science policy I interpreted to concern the role of the social sciences in the formulation and direction of science policy. You [Jevons] interpreted it to concern the study of science policy. What role do you see the social sciences playing in the formulation and direction of science policy in the way that natural scientists do tend to be involved in the upper levels of formulating science policy?

**Jevons:** Do you mean practitioners?

**Questioner:** Yes. It would not have surprised anybody if ASTEC had only natural scientists appointed to it. It would be quite natural — in fact it would be one of the natural laws of science policy formulation. I thought the question was more about what role you saw for the social scientists in the formulation of science policy.

**Jevons:** That is part of the bigger question on the role of social sciences in any policy making. Is it the proper route into politics to study political science at university? I would say that there is a big question mark over that one. Do you play the game at your university of asking, which department is worst at so-and-so? You can start off at Manchester by asking which department has the least liberal attitude towards science. Then you can go on to ask which department has the worst piece of architecture on campus? Which has the most psychologically unbalanced people in it? Which understands its social structure the least? And then you come to the question, which is the worst governed in the university? The answers to these questions can be somewhat embarrassing.

I wonder if we could get Bob Robertson to enlarge on what he terms ‘rational thought’ as a form of thought which non-scientists are often not capable of achieving?

**Robertson:** Let me begin by saying that not all scientists are capable of rational thought, nor is the desirable way of tackling the problems of the world confined to scientists. I do not mean that for a moment.
Many lawyers argue with the same sort of logic as scientists, even though they often do not start with the same premises. I think that the short answer to what I mean by rational thought is that we are tending to solve the problems of the world or of the community by the use of scientific techniques of investigation and experimentation. Rational thought is what you use to interpret the results of your investigation and experimentation. We are trying to do that without prejudice arising from our particular backgrounds. I think if you go back into the 19th Century it was very difficult to find many people in Western countries who were in any way rational about the relationship between the coloured races and the white races. I think now that there are many more people who are rationally trying to think this out — sometimes disagreeing — but the attempt is not being made on the basis that 'I have always believed that black people are incapable of doing the things that we are capable of doing' which was a common 19th Century attitude. And I take that as one example of the way in which there is rational thought — not thought on the basis of what had been said in the Bible by God or on some other pre-conceived basis. Do you agree?

**Jevons:** I do get a bit worried when people talk about applying 'rational' or 'scientific' methods to social problems because it reminds me so vividly of, for instance, what Bernal was writing in the 1930s. He conceived a technocratic Utopia which is not my kind of Utopia at all, in which social problems are always investigated in 'rational' ways largely by the use of numerical techniques. I perceive a strong swing away from that kind of view on how an ideal society would run. I have tried to express this in one of my lectures (Chapter 3) as a weakening of the belief that the problems of society will be solved by getting more scientists into managerial and administrative positions. To that extent I would go along with the anti-science brigade. I do not think that kind of 'sciencism' is the answer to the problem at all.

**Robertson:** I do not either.

**Jevons:** Bernal wrote in about 1949 — not all that long ago — that he sees the 1—2% of scientists in the population going up to about 20%. That necessarily involves a broadening of the definition of scientist — Bernal says it would include both scientific production and scientific administration. I wonder very much whether you and I
would want to go along with all the presuppositions inherent in Bernal’s notion of ‘scientific administration’.

The Liberal Studies in Science course at Manchester University

Evans: I was very impressed by your description of Liberal Studies in Science at Manchester University and I must say that we are chasing after some of the same aims here at ANU. I feel that about 100 different things at the present time point to the need for making, as Ashby\(^3\) says, politicians more aware of science and scientists more aware of politics, which might be broadened to say economists need to be more aware of the natural sciences, scientists need to be more aware of society and the social implications of science. There is the doubt and worry of whether students will be recruiting to the pure sciences (meaning the natural sciences). There are the increasing worries about environmental crises — the implications of new technological innovations. It seems to me that all these things point to a need for the broadening out of university science education. I therefore find it incredibly out of tune with the times that, I imagine, something like 90% of scientists still do a pure science degree at the undergraduate level in Australia. I myself feel that the change to scientists learning more about the social implications of their subjects at universities is not happening fast enough. I was wondering if you would like to comment?

Jevons: I think I agree. If you look through the lists of courses available you might think that a great deal of reform has taken place but if you look at the number of students going through the various courses the picture is very different. In Britain the great majority still go through more or less standard single discipline courses. There is a certain amount of change in the courses in that they tend to have social aspect components or environmental components in them.

Evans: Has anyone copied your course in Britain?

Jevons: No one has copied our precise model but there are lots of other ventures aimed at broadening science courses. I am thinking in particular of the ‘environmental’ courses of which there are now dozens in Britain. When we were starting in 1966 we were almost the
only ones offering a broad science-based education but there are
now lots of other groups with names quite different to ours in the
broad science field. I have never been very enthusiastic about the
joint honours approach which puts together two different science
disciplines because I am sometimes left in the dark about what the
justification is for putting Science X together with Science Y and
calling that a broader education. It seems to me that the justification
of putting a science together with something social is much clearer.
Aren't there many environmental courses on the Australian
undergraduate scene? There is one at Griffith University, isn't
there?

Evans: Two new universities — Griffith and Murdoch both have
whole schools devoted to environmental studies but it looks as if they
will be training environmental specialists.

West: Do you think their graduates will be able to get jobs?

Evans: I think the same thing might apply to what Professor Jevons
was talking about with his own graduates (Chapter 4). The graduates
from Griffith could be so specialised that they will be looking for
jobs in the Department of the Environment. I would prefer to see
something broader than that. In the older universities — that is all
the others apart from Griffith and Murdoch — there are very few
courses at the undergraduate level that permit a scientist to look at
the social and other limitations of his science. For instance there is a
single course in the third year at Flinders University which is a
committee course run by someone from here and someone from
there. There is also a course in the first year at Sydney University on
environmental studies but these are just isolated courses. There is a
general assumption here that one should be well socialised in one or
more disciplines first and then one can safely turn coffee table
discussion into a professional course. Therefore Macquarie and
quite a few universities have graduate diploma and Masters' degree
programmes in environmental science or in environmental studies.
The undergraduate area is still very much disciplinary.

Jevons: As far as training 'professional environmentalists' is con-
cerned, the official position in Britain is that the best preparation is a
good solid safe first degree followed by an 'environmental' Masters'
course. Quite separate from us at Manchester, there is an M.Sc.
course in Pollution and Environmental Control (see Chapter 4).
Evans: I think it is valuable to recognise two distinct functions that environmental educational programmes can serve. One function is to produce environmental specialists and the other function is broadening people.

Jevons: Can we draw Professor Fenner into the discussion?

Fenner: I am at the learner stage as far as environmental education is concerned. The Centre for Resource and Environmental Studies at ANU is going to tackle it at the Masters' level partly as a result of the way we are structured and partly because the Human Sciences Programme is running at the undergraduate level in the School of General Studies. We hope to do very much what they are doing at Manchester, taking people who lack natural science training and with qualifying courses (which will be either part-time courses for Canberra residents or intensive bridging courses) to give them a partial education in the aspects in which they are interested. At least they will then be able to understand a little bit of chemistry, a little bit of ecology, a little bit of economics up to say senior high school level and then have a course-work Masters' degree with a variety of different options; every student's course would probably be different. There will be only a small number of students — a dozen at a time. The detailed planning awaits the arrival in January 1976 of the man we have appointed [Mr David Smith from Bristol University]. The course will commence in 1977.

Jevons: Environmental science and environmental studies are clearly in an emerging stage at the moment — not yet fully professionalised or institutionalised. Environmental studies units inevitably draw into them people from other disciplines. Do you see this as a transient phase? Do you foresee a stage in ten or twenty years time when it becomes fully professionalised and people will be trained in environmental science and practise as environmental scientists?

Fenner: I think I have some misgivings. These misgivings were expressed quite specifically about a year ago when Professor Ken Hare was out here from Toronto. He had been advisor on scientific research to the Department of the Environment in Canada for a couple of years, and had been looking over the environmental programmes of the universities in Canada. He was quite firmly of the opinion that for training environmental scientists it was better to have a first degree in a disciplinary field, which did not preclude the
possibility that you got a broad education as well. I think that is what Jeremy [Evans] is talking about — a broad education provided in parallel with a disciplinary education. Hare thought that the best training for environmental scientists who want to go professionally into that area is that sort of first degree and then a graduate qualification in environmental studies — Diploma or Masters — usually by course-work because there is a lot of material and information to be presented to the students. I think it is at York University in Ontario, where they have a large environmental science programme for undergraduates, that there is a problem in the employment of graduates — who had no further training than their first degree. There were plenty of opportunities for help with pollution problems or to assist the shire engineer, but it was found that the man whose only training had been very broad was not as acceptable to employers as the person who had first trained for example as a chemist or as an engineer, and then subsequently had trained in a broad course.

**Steele:** When Professor Gardiner from the University of California, Riverside was here recently, he said virtually the same thing. They used to have an environmental course in the Department of Chemistry which, after operating for about five years, was phased out altogether for just that reason. The people that went through could not get jobs. There was a feedback mechanism — the word got around that if you took that course you would not get a job — it was as simple as that.

**Jevons:** Do you mean jobs as environmentalists or jobs of any kind?

**Steele:** Apparently it was a bit of both — there were not sufficient openings in jobs as environmentalists and they were less acceptable for other jobs such as those in industry.

**Webb:** Surely that would not include jobs such as teachers of environmental science in high schools. One would imagine that would be a particularly large area of employment.

**Jevons:** Is ‘Environmental science’ available as a Higher School Certificate subject?

**Webb:** I do not know about America but it is just coming in Australia.
Jevons: It is just getting off the ground in England as an 'A' level subject — there are two or three experimental schemes.

West: At the University of California, Riverside, they now offer a course in analytical chemistry which uses examples from the environmental area. Students get a basic foundation in analytical chemistry which seems a better way to approach the problem.

Steele: Yes, the course is called something like 'The Science of Measurement' and they really put them through the hoops in analytical chemistry.

Centre for Research on Science and Technology Policies

West: If a Centre for Research on Science and Technology Policies was being established and you were the Vice-Chancellor, what would you expect from such a Centre? And if you were the head of the Centre, what would you study?

Jevons: If I were a Vice-Chancellor I would like to be able to say that my University has a Centre studying the problems of how much society should invest in scientific research and in what kinds of research it should invest. These are the questions that people want answered. But if I were actually the head of the Centre, I would not tackle them head on because they are impossible questions to answer. I would try to get down to brass tacks. I would certainly try to be active in the field of the organisation of scientific research, bearing in mind all the arguments during the last couple of months about CSIRO. I think I would avoid the top levels of CSIRO if I were starting a research programme on it and look instead at the grass roots. One could study, for example, information flows. How do the bits of information actually flow into and out of CSIRO between scientists and users? One could look at research establishments wherever they are — whether in CSIRO, university or industry. Technical information mainly flows out and user information and market information would flow mainly inward. Do we know anything about the details of these flows?

West: Very little. One or two projects have been studied in greater depth; for example, the atomic absorption spectrometer has been studied by a number of people. When you get to other developments in CSIRO we don’t know very much about them at all.
Jevons: Studying a few big innovations could be widely misleading. You can get seriously misled about technological innovations if you focus only on nuclear energy, for example.

Robertson: I think CSIRO in different sections really knows a lot about this. It has not perhaps been formalised so that you can go and look up a book as to what the research stimuli were, what the outputs were, what the applications were, but, in different bits of CSIRO, I think we do know. I speak as a grass roots CSIRO man of 25 years ago. We did know, in my area, exactly who was working in fruit storage research in Australia. We had yearly meetings of the people involved in fruit storage research. Each of those people who came to those meetings was, to my knowledge, in contact with the fruit storage industry in his particular fruit and in his particular area and then we added New Zealand to this whole scheme. And if you wrote a book about what is in the reports of those meetings and those committees, you would have a great deal of information about the whole exchange of information from people who had research ideas through to change in the fruit storage industry. I think the same would be true for other sections of CSIRO.

West: I tend to agree with you. I am sure that those working for CSIRO from the top to those at the bench would be able to tell you something about the information flows. The Advisory Committee is also probably an important element in the scheme of information flow into CSIRO.

Muecke: The Advisory Council does have a certain degree of initiative. Professor Fenner’s report to the Executive on medically related research in CSIRO is just one example. Another example is in new lines of research in manufacturing industry suggested by a small committee of the Advisory Council. To some extent it is a public relations body. There are also State Advisory Committees (in all states except NSW) whose job it is to have a look at State-related programmes in CSIRO and to give a State opinion. I cannot say from my own experience how effective the State Committees are although their opinions contribute to CSIRO’s thinking on research planning.

Jevons: What people have said has confirmed my feeling that the study of information flows is a worthwhile project for a body like the proposed Centre for Research on Science and Technology Policies at
ANU. Although material for the study is available in different places to people actually involved in fields such as fruit storage, I do not think there is an overall view. I would be interested to know how the relationships vary from area to area.

*Muecke:* They vary enormously.

*Fenner:* As an extreme example, I was impressed in Brisbane last May with the degree to which the Meat Research Laboratory is in touch with industry. CSIRO has representatives in all killing works in tropical Queensland and the interchange of information on the handling of carcases is almost immediate between the laboratory and the centres where the slaughtering is being carried out. The changes in procedures which could otherwise take 10 years to filter through are achieved in a very short time. In other places there is a 10 year delay. It does depend very much on the initiatives of both sides, research and industry.

*Jevons:* I would want to know how thing vary with the kind of knowledge and the structure of the user. With an agricultural user the situation must be vastly different from that with an industrial user.

*Robertson:* I expect an extension of what we are just saying is that there will be a good bit to be learned from research on what has happened in State Departments of Agriculture which are very much closer to the users than CSIRO and sometimes do the essential applied research themselves. At other times they use the CSIRO research and bring it through to practice with the farmer who is ready to accept it. I think that probably the history of that would be very valuable in saying what the difficulties, mistakes and gaps are in the research on the Australian scene in relation to the application to the Australian scene. I suspect that there is something also to be learned from the relationships of the mining industry of a few years ago to the State Department of Mines.

*West:* I also think that the information flows in Australian government laboratories apart from CSIRO should be examined. For example, what are the inputs and responses to those inputs in laboratories such as the Aeronautical Research Laboratories?

*Drabble:* I think CSIRO has done some studies in the agricultural
extension area on how research results are getting through to the farmer. I heard a talk from the people who publish *Rural Research in CSIRO* — a publication aimed at agricultural extension officers. They had carried out market surveys and the results they revealed were basically depressing. *Rural Research in CSIRO* is not read at all widely — it is read with great enthusiasm by the wrong population. It is well received in schools, by other scientists and by a variety of laymen. It is not by any means the source where agricultural extension officers draw their material. The knowledge of extension officers and many farmers seem to be drawn from the advertisement pages of the journals of the State Departments of Agriculture. The people involved with *Rural Research in CSIRO* tell the anecdote that they were very much bucked up by an enthusiastic response to an article on 'Energy down on the farm' by somebody with a farming address but they were depressed when they found he was a local admiral dealing with a hobby farm.

**Study leave for Australian research scientists**

*Jevons*: So as not to make myself too unpopular, I will suggest this research topic just as I get on a plane back to England. The topic is to investigate whether the generous provision of study leave for Australian research scientists in fact does more harm than good to Australian research.

(*much hissing*)

*Muecke*: Could I just make a comment here that sabbatical leave has changed its form in CSIRO in the last decade or so. The common experience now is to take an extended trip and not to visit another laboratory for a long period. Most CSIRO scientists travel very extensively, spending a few days at individual laboratories and trying to cover as many as possible — trying to take in one or two major conferences on the way. And overall the period of absence is a matter of months and perhaps less than half a year. I don't know if this is necessarily a preferable arrangement but it is something that is available and I suspect it is seen to be of value to CSIRO people. Mind you, they do not have the aspirations of getting away from teaching duties to get more time for research.

*Kefford*: Perhaps I can give you an example from the U.S. My accent indicates that I am Australian but I am visiting from the
University of Hawaii. During the early 1970s a very distinguished group of scientists from the American National Academy of Sciences reviewed agricultural research throughout the United States and they came up with a rather damning report, not in the sense that agriculture shouldn't have research but on the quality of research currently occurring in agriculture. And they compiled a list of the desirable attributes that you might find in agricultural research institutions. One of these was travel for retraining and for contact with other scientists. This is a desirable and necessary attribute. One of the things I am doing here is talking to people in institutions around Australia and one thing I came across commonly is the concern that there is not enough flexibility in manpower resources in institutions. The needs of research change but the director is really stuck with people who are there because they have tenure. What can you do about it? One way you might think of this is in terms of retraining. Perhaps a new requirement of universities is to pick up people who have been in research for some time and to change them slightly or more than slightly. Certainly travel and retraining are very important aspects for us to take into account.

**Brewer**: I would like to make a comment rather than ask a question. I have been a statistician for the last 20 years or so and most of that time has been in the Australian Government. We have found that to a very large extent we tread the same statistical treadmill. It isn't until one of us goes overseas and brings back ideas from overseas that we really experience any increase in our professional skills, with the exception of course of some ideas that we have produced in our own area and which are completely ignored overseas until we go overseas and talk about them. Australia is a very isolated place — at least statistically and . . .

**Unknown wit**: Do you mean to say that Australia is moving around — sometimes it is here and sometimes it is there but usually it is where we all believe it to be in relation to the rest of the world?

**Brewer**: I mean to say that there is very little coming and going as far as government statisticians are concerned. Chiefly I guess because of the very careful watch that the Overseas Travel Committee has on the overseas travel of public servants. I am a newcomer to the University but I think it would be a terrible thing if the travel provisions that Australian scientists and academics have were to be cut out from that point of view.
Jevons: I can't help recalling the old joke about the headline in the London Morning Post: 'Thick fog in Channel. Continent cut off'. I think you have the same problem — who is cut off from whom? Australia is full of high grade manpower. They have facilities that are as good as anywhere in the world. I wonder how much good research is not being done in Australia because so many of the best people are spending so much of their time on planes.

Robertson: I would like to suggest that the trend in university study leave has been rather like what Peter Muecke described for CSIRO. I have no statistics, but my impression would be that more people are tending to go for shorter periods to make contacts rather than having the whole year away. I think there is an important problem that differentiates the Northern Hemisphere from the Southern Hemisphere. While we do have a fair number of people passing through in Australia, it is much rarer to have a visit from someone in the same line as you are than if you are in the U.K., a European country or in the U.S.A. On that Northern traffic route every scientist meets a specialist who is seeing other specialists in the field about once a year if not more frequently. In those circumstances people do keep each other up to date. If Australian study leave is used for the equivalent of going to the Northern Hemisphere, going round seeing people and getting your ideas straight and then getting on, it might be a good thing. In those circumstances people do keep each other up to date.

West: One advantage of going overseas is to give you more confidence in yourself. For example I always had the impression that facilities in other parts of the world must be much better than in Australia. However after visiting laboratories overseas I realised that this is not the case.

Other suggested research topics

Jevons: There are three other areas in which I think there should be more work. Firstly, I think there is a case for carrying out international comparisons. We have talked about comparing different sectors of Australian research but there is also the possibility of comparing Australian research with other countries' research. The obvious country with which to compare Australia is Canada because there are so many similarities between the Australian and Canadian situations.
Secondly, research on industrial research management. There isn't much of it going on in Australia and some people are asking why there isn't more. It is less a scientific problem than a managerial problem. There is in fact an incipient group of research managers in Australia (The Australian Industrial Research Group). They meet mainly in Sydney and Melbourne and I do think that something might be done in conjunction with them.

Thirdly, technological assessment — whatever that means.

Drabble: I would like to comment on technology assessment with reference to some earlier remarks. I was wondering, when we were discussing the coupling of CSIRO and industry, whether sometimes the coupling may well be too close. In other words, the beef scientists may be so closely coupled with the beef industry that it might mean that the whole organisation itself was unable to see that a broader approach to an agricultural system might be more appropriate. But a lot of funding for CSIRO comes from industry sources — for example from the wheat industry specifically to do research on wheat. It may well be that in the regions on which the scientists are carrying out the research, other crops such as sorghum, which could be either sold or fed to cattle on the farm, may be more appropriate. However, it is the wheat farmers who are financing the research. I think that this close coupling of the industry to the research may not be highly desirable.

West: The dependence of CSIRO on industry funds is not as great now as it was previously as the proportion of funds from industry for agricultural research has declined in recent years.

Drabble: The proportion of funds from industry may in fact be declining but the influence is not necessarily declining. An enquiry on how rural research should be financed is in fact being conducted by the Industries Assistance Commission at the present time. It is being conducted by social scientists — by economists — and a lot of the evidence . . .

Robertson (interjecting): One scientists — Dr Melville.

Drabble: They have suggested that the lines of research on regions or agricultural systems being pursued by CSIRO scientists have been tied more or less by the direction of the funding bodies. In fact the drop in funding might mean that there is a multiplier effect. Even
though the funding bodies might provide only 10% of the funds, they 
would appear to influence the other 90% on some occasions. Would 
you like to comment on the fact that coupling can be too close?

*Jevons*: Can coupling be too close? Yes, that is one of my Vice-
Chancellor’s problems because it reduces itself, if I have interpreted 
the question correctly, to the extent that you want basic research 
going on. As far as I know there is no way of tackling that problem. 
You see, basic knowledge flows across international frontiers more 
readily than applied knowledge.

**Research versus practice**

*Kefford*: The amount of research done in industry in Australia is one 
thing that concerns me. I have to do with agricultural research, 
biological research, and in the U.S. there is something like US$1,000 
million spent on agricultural R & D and half of that is in industry. I 
do not know what the figures would be here in Australia — I would 
expect that they would be rather less than that.

*West*: It would be less than 10% I am sure.

*Kefford*: The Land Grant system in the U.S. has educated people to 
make them sell themselves to industry. I think we train students very 
well to perpetuate universities but that has gone out of fashion — 
there aren’t many new universities coming up. I don’t know the 
solution — all I can do is worry about it. How can we train our 
students better so that they have the wherewithall to go and sell 
themselves to industry and once more be effective there and so 
encourage industry to employ more graduates?

*Jevons*: Can you elaborate on the significance of the Land Grant 
system in the U.S.? 

*Kefford*: The Land Grant system was created in the United States in 
1862. It was a new concept of university education, although it now 
seems quaint to us that it should have been new. The concept was to 
educate people in useful things. It was opposed by the likes of the 
 Presidents of Harvard and Yale because in their philosophy of 
education it was not included and what’s more they thought there 
had to be a society with classes in it, including some that weren’t 
educated. The new philosophy was that education had to be useful,
and this is the philosophy of education of universities like Purdue, Cornell, etc.

Robertson: I think Ned [Kefford] touched on a very important point about Australia which seldom gets mentioned. We tend to educate in universities people that would be good at education in universities because we give them the mores that we science teachers at universities have. Included in that is the assumption, not often stated but frequently made, that the greatest thing in life is achievement in pure research — the purer it is, the greater the achievement. Although I am obviously overstating the case, nevertheless, it is true that in Australia most people who have done a Ph.D. degree want to go on doing research. They do not want to go out into industry because the people they most admire are not the ones that are in industry. I think that this is an interesting sociological problem. A few years ago I took out the figures for Ph.D. graduates going into industry in Australia and it was 1 in 18. That includes all the chemists that you might expect would go into industrial jobs and it includes industrial jobs of any kind, managerial or research.

West: A lot of people talk about it but not very many do much. If you do try like I have tried you don’t get very far. For example, I wrote letters to many people (the Minister of Education, the Australian Vice-Chancellors Committee, the V-C. at ANU etc.) to try and get some break between the first degree and the Ph.D. Some universities now allow such deferment but the University of NSW is the only university which does it to any extent. The Department of Education is not happy to change their scholarship regulations but want the universities to make the changes. Apparently the Department is quite prepared for the universities to offer students a scholarship and allow them to defer it to a subsequent year. The universities will not advertise this fact — you have to say after you have been offered your Ph.D. scholarship that you would like to defer it. Then they will tell you about it — they will not tell you about it beforehand. A lot of people who defer will not go and do a Ph.D. subsequently but that doesn’t matter because you are getting highly skilled people going into other areas. The second thing I have tried to do is to introduce one-week bridging courses like the Science Research Council has in Britain — I have managed to have four Australian students in the U.K. doing Ph.D.s go on these courses — I haven’t got any money to pay for them yet — we don’t have to pay
until September. I am also trying to get the idea taken up in Australia. The Academy of Science has not been particularly interested in either proposal.

The Australian Academy of Science

Webb: It is a disappointment that the Academy is not taking a bigger role in this sort of thing. The Research School of Chemistry brought Professor Peter Fensham from Monash last week to talk about the Hill, Fensham and Howden Report on Ph.D. education. That is a report that really has a tremendous number of things to say, yet it has already floated off almost into oblivion. It has not had nearly the impact it really should have had. In a way I wonder if in fact the Academy of Science is not in part to blame.

Robertson: I completed a term a year ago as President of the Academy of Science and I am not going to defend it but I think and I hope I am going to look objectively at the problems. And this is related to what I said earlier. I think that bodies which separate people off for their stature in an area in which they admire each other ought to be on their way out in the modern community and I think bodies like the Academy of Sciences, the Royal Society of London, the National Academy of Science of the United States will go out in time. In China, where they do things revolutionarily, it has gone out — you no longer get elected to the Chinese Academy of Science as you used to. This is related to what I said about the pecking order being wrong. The Academy, including myself, has been too pure and it is still pure and it does not effectively interact with the wider scientific community. I have said this publicly in the Academy and found that not all of my colleagues agree with me, but I believe that we do not interact effectively on just the sort of problem you have raised because we are too small, we do not have enough people except old buffers who are over-committed anyway. The bridging could be done if we were a bit more effective in the way the US National Academy of Sciences is. The US National Academy has about 800 members and the number of people that you find in their annual reports doing committee work for them is about 8,000. They have committees on science in every possible way — sometimes very vocal, sometimes very effective, and sometimes ineffective. I do not think the Academy of Science is a barrier to the progress of the scientific community but I think that some of the activities that we have just been talking about have to be carried by the scientific
community as a whole — not merely those whose research accomplishments have been such that you can clearly recognise them as people who have accomplished a piece of research. The difficulty is to give honours to those who haven't accomplished a piece of research. I don't know if Frank [Fenner] disagrees with me or not.

**Fenner:** No, I go all the way with that.

**Jevons:** I would like to know more about the alternatives. You have described bodies like the Academy and the Royal Society as mutual admiration societies which are self-selecting but when you have more democratically elected bodies my impression is that they are even less successful. Did not Japan have a body which was elected by all practising scientists which proved to be ineffective?

**Robertson:** I think that is just the common problem of democracy.

**Kefford** (interjecting): You get Jimmy Edwards as provost.

**Robertson:** I think that the key is the interaction of small groups of active people. In the chemical field, the Royal Australian Chemical Institute does a lot of worthwhile things the Academy of Science would never be able to do. The RACI is a broad body with a large membership and can do things effectively. The Society for Social Responsibility in Science is another very important input. I think it is important that there is a variety of interests and a variety of people that interact with each other and criticise each other.

**Jevons:** Then we want to know what organisational measures will encourage the formation of these small interacting groups.

**Muecke:** Could I just add a comment. The example of the US National Academy is a good one as that body was commissioned at its formation by the US Congress to do just these sorts of things — to report on anything on which it was asked to report by the Congress. Apparently for many years nothing was asked of it and it didn't do anything until this provision was recalled (I believe at the time of the First World War). From then on it has been a major force in science policy matters in America. I suspect that ASTEC could arrange the same sort of standing committees to do the same sorts of activities.

**West:** The Academy could also do more if it had more money.
References

In September 1972, the then Vice-Chancellor, Sir John Crawford, issued an invitation to staff and students at ANU to submit suggestions on academic developments for possible inclusion in the University's submission to the Universities Commission for the proposed 1976-78 triennium. Dr West proposed that a Science and Technology Policy Research Unit be established. As a result of a certain amount of interest within the University, the next Vice-Chancellor, Dr R. M. Williams, established, early in 1973, a working party to examine the proposal consisting of Professor A. J. Birch, then Dean-elect of the Research School of Chemistry; Sir Rutherford Robertson, Director of the Research School of Biological Sciences, and Professor D. A. Low who at that time was Director of the Research School of Pacific Studies.

The working party recommended on 11 July 1973

(a) that the Advisory Committee on Science and Technology Policy Research be appointed by the Vice-Chancellor as soon as possible to advise on all matters relating to work in this field by the University;

(b) that the Advisory Committee have about seven members and initially to consist of seven people who had showed interest in the proposal;

(c) that sufficient funds be made available to enable one or two visiting appointments to be made in 1973-75;

(d) that $50,000 per annum be requested for 1976-78 to meet the costs of making further visiting appointments and possibly one or two academic appointments;

(e) that a final decision on the development of research in science and technology policy in the University be made in connection with planning for the 1979-81 triennium

These recommendations were endorsed by the academic boards and by the Council of the University and funding was sought from the University Commission in the University's Submission for the 1976-78 triennium. The submission contained the following paragraphs:
4.5.5  The University has noted the small amount and sporadic nature of work done in universities in research into science and technology policy. An understanding of the factors in the field which will affect policy both in the government and private sector is seen to be of great importance.

4.5.6  The University has therefore decided that it should seek an amount of $50,000 a year in the 1976-78 triennium (a total of $150,000 for the period) to enable an advisory committee within the University to develop a program of research and action aimed at gaining an understanding of the factors concerned. The University intends that these funds be used by a committee to investigate the ways in which a continuing program might be established beginning in the 1979-81 triennium. It expects that the committee would arrange a number of seminars and discussions, which would be enriched by the presence of visitors from overseas. It may also be appropriate to make one or two short-term academic appointments and to seek co-operation with existing work being done in universities and government in Australia."

The Advisory Committee on Science and Technology Policy Research (ACSTPR) held its first meeting on 1 March 1974 when Dr West was elected Chairman. Since that time the Committee has organised workshops on the Australian Government's Green Papers on the proposed Science Council (30 July 1974) and Antarctic Research (28 May 1975). It has also carried out a study on course patterns of Science graduates at ANU as a prelude to studies on the supply of and demand for scientific manpower in Australia. It is expected that the results of the course pattern study will be published early in 1976. Money has been allocated to support a study in the Department of Sociology in the Faculty of Arts at ANU on the mobility of Australian academics. The Committee arranged the visit of Professor Jevons to ANU and this book is an outcome of that activity.

Much of the work of ACSTPR has been planning the establishment of a continuing academic grouping within ANU to carry out research and associated activities in the science and technology policy area and to seeking outside support for such activities. In July, 1975, ACSTPR proposed that the Centre for Research on Science and Technology Policies (CROSTPO) be established at ANU in 1976. With the postponement of the commencement of the triennium from 1976 until 1977, consideration of the establishment of CROSTPO has been delayed
until the result of the Australian Government’s consideration of the Universities Commission’s proposals for the 1977-79 triennium is known. This will be at the time of the Budget in August or perhaps earlier.

It is envisaged that initially CROSTPO will have three academic appointments and that it will continue the visitors programme commenced by ACSTPR by having two Visiting Fellows for up to three months each year. Two major research areas have been proposed — (i) scientific manpower studies and (ii) the examination of policy alternatives for government involvement in research and development in Australia. It is not envisaged that the Centre will initially be involved in the organisation of undergraduate teaching although a part-time Masters’ Course in Science and Technology Policy Studies could be commenced at an early stage. Short courses (approximately one week in duration) in several areas have also been proposed.
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In mid-1975, Professor F R Jevons visited The Australian National University under the auspices of the ANU Advisory Committee on Science and Technology Policy Research. The purpose of his visit was to stimulate interest in the study of science and technology policy issues.

During his stay Professor Jevons gave a number of lectures covering the relationship of basic scientific research to technological innovation in industry, the organisation of scientific research, the nature of knowledge, and science in literature. The lectures and other material, edited by Dr C E West, are now made available to the wide range of people interested in the relationships of science, technology and society.

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Professor Jevons obtained his Ph.D. in Biochemistry in 1953 from Cambridge. He has held various appointments in Biochemistry at the Universities of Washington (Seattle), Cambridge and Manchester, and in 1966, was appointed first Professor of Liberal Studies in Science at Manchester University. The interests of the department developed by Professor Jevons centred upon an examination of science from the economic, social, historical and philosophical standpoints. In 1975, Professor Jevons was appointed foundation Vice-Chancellor of Deakin University at Geelong in Victoria. He has written three books and contributed to three more.

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